Appendix J

Staff Responses to the Major Comments Received from the University of California Peer Reviewers Regarding the CARB Draft Final Report

CARB received comments on its draft final report titled "Lake Tahoe Atmospheric Deposition Study" from Peer Reviewers selected by the University of California, Office of the President. Comments were received from Professors Thomas Cahill (UC Davis), Keith Stolzenbach (UC Los Angeles), Gail Tonnesen (UC Riverside), Akula Venkatram (UC Riverside), and Tony Wexler (UC Davis). The comments of the peer reviewers are presented in their entirety in Appendix I. The more substantive critical comments of the peer reviewers are repeated here in Appendix J with the staff responses interspersed (shown in italicized font).

Summary of the Major Comments Received from Professor Thomas Cahill with CARB Staff Responses

General Comments

Comment: LTADS, by far the largest air quality study ever mounted at Lake Tahoe, represents a major enhancement of our knowledge of Lake Tahoe air quality and a major advance in our understanding of deposition phenomena. Because of its size and scope, it should be able to stand alone as a primary source for present and future planning and the basis for additional research. As it is written, it does not reach the stature I would have expected from such a massive effort. I recommend that additional funds and about a year should be taken to merge this study with prior and concurrent data.

Response: *CARB commitments and priorities to additional, more critical air quality issues did not permit a comprehensive integration, analysis, and summary of all air quality work in the Tahoe Basin. The report primarily focused on LTADS itself and what new information it brings to bear on better characterizing the atmospheric contribution to Lake Tahoe's declining water clarity.*

Action: *CARB staff, with the assistance of the Tahoe Regional Planning Agency, sponsored a seminar on LTADS in December of 2005 to help convey what was learned during LTADS, what additional analyses the LTADS database could support, and what additional research will be required to address remaining air quality issues.*

Comment: While the DRI XRF and UCD S-XRF data are in excellent agreement for major elements before particle size self absorption corrections are applied, (example: 0.99 ± 0.06, $r^2 = 0.94$ for silicon), the phosphorus data used by LTADS, when corrected for self absorption of the phosphorus x-rays in a soil matrix, are way too low…

Response: *Staff concurs that the two XRF methods probably agree for the major elements. However, the UCD laboratory details regarding field blank and self absorption correction factors (SACFs) were not provided to permit independent confirmation. The plots comparing the sulfur (S) measurements look excellent for both the ambient and source samples. The silicon (Si) comparison plot for ambient samples shows a good 1:1 agreement while the limited source sample plots do not. The calcium (Ca) plot is consistent with the 1.23 SACF that DRI used, assuming UCD did not apply a SACF. Comparisons were not provided for aluminum (Al). Staff agrees that s_XRF provides a more sensitive measurement of P than standard XRF and that the standard analytical method used in LTADS yielded very few P detections. Based on the s_XRF results for a sub-set of the total sample population and an analysis of the measurement uncertainties and detection limits, staff chose to use a spatially and temporally constant P concentration (20 ng/m^3) in the initial P deposition analyses that were peer-reviewed. As Professor Cahill's more recent theoretical analysis indicates, SACFs increase rapidly with size. These measurement corrections and uncertainties not only raised the concentrations reported but also the detection limit of the XRF technique.*

Action: *Because Professor Cahill's analysis has not (as of the preparation of this final report) undergone peer-review and the SACFs are large and very dependent on the size of the particle* *and the assumption about the distribution of P (and other species) within the particle, staff elected to increase its best estimate of P concentration to 40 ng/m^3 based on multiple analytical approaches that indicated P concentrations between 25 and 55 ng/m^3.*

Comment: Only limited efforts were made to compare the year of the LTADS study to prior years at Lake Tahoe to put the study into historical context. Such a context is essential.

Response: *Meteorology and the subsequent dispersion of pollutants is a very complex process. Staff did include a comparison of precipitation amounts in 2003 with the climatological norm. A better indicator than precipitation amounts is the number of days with precipitation but staff did not find an existing climatological summary for that statistic. Climatological summaries exist for maximum and minimum temperatures but temperature is more loosely related than precipitation to ambient concentrations.*

Action: *Staff found some, and created additional, climatological norms for the number of precipitation days by season and compared the 2003 results against them. Staff also developed monthly means of the 2003 maximum and minimum temperatures for comparison with existing climatological temperature norms.*

Comment: ARB and its contractors did not access or even list an enormous body of useful prior work at Lake Tahoe, including early ARB and USEPA work, TRPA sampling 1988- present, including archived IMPROVE samples and the much of existing data from ARB site at Sandy Way. Some of this prior work conflicts with the present studies. The net result is to greatly enhance LTADS uncertainties.

Response: *Staff concurs that prior research efforts in the Tahoe Basin have generated substantial data with various sampling methods of varying quality and consistency. Staff made substantial use of those data for filling critical gaps (e.g., vertical temperature profiles, wind frequencies) and comparing results from LTADS (e.g., IMPROVE and FRM PM sampling results, denuder measurements of nitric acid and ammonia). Some of the historical data are not internally consistent or of uncertain quality (many of the measurements challenge analytical capabilities and represent limited locations for a brief time). Staff elected not to rely on the land-based deposition buckets or conclusions drawn from those observations for estimates of dry deposition. Temporal variations in the site characteristics of at Wallis Tower (e.g., the variation in the influence of vegetation near or over the buckets) make interpretation of those observations uncertain. In addition, staff has questions regarding aerodynamic effects caused by the sampler itself as well the effects associated with the modification to a water-based sampling method, including effects introduced by insects, birds, and tree pollen. However, the bulk surrogate surface measurements on the Lake during periods inclusive of precipitation events could reasonably be used as an upper limit estimate of wet deposition to the Lake because the measurement include both wet and dry deposition. Staff focused their LTADS efforts on providing sufficient additional and pertinent meteorological and air quality data to effectively address air quality issues and to support estimates of dry deposition based on observed (or estimated where necessary) atmospheric concentrations and deposition velocities modeled from meteorological data. This approach is independent of most previous work and can be used to* *confirm or challenge the conclusions or hypotheses drawn from pre-LTADS data. The purpose of the LTADS report is to describe the measurements from the field study and to release the data to analysts, planners, and the general public to inform decisions and actions that might be taken.*

Action: *Other than clarifying existing statements that could be misinterpreted, staff invested no additional efforts toward a comprehensive compilation and integration of previous work in the Tahoe Basin.*

Comment: Uncertainties, especially for species like phosphorus, should be broken out into the specific categories before being combined into a single number. This should include uncertainties in ambient measurements (siting, sampling and analytical uncertainties, year to year variability, sources not considered,…) and deposition modeling (averaging and extrapolations, surface roughness and water deposition factors, inversions,…), perhaps including examples of prior efforts at deposition estimates.

Response: *This statement implies a detailed understanding and the existence of extensive measurements to precisely quantify each of the uncertainties in a complex and dynamic system. Characterization of the cumulative and general uncertainties may be the best that can be done. In particular, staff has limited confidence in the ability to precisely quantify ambient phosphorus concentrations by any analytical method.*

Action: *Staff increased the discussion of uncertainties and, where feasible, included improved characterization of the uncertainties. Staff also expanded the comparison of deposition estimates from LTADS with other sampling methods.*

Comment: The use of an upper cut point (TSP?) for the TWS eliminates particles and may eliminate coarse dust from roadways and strong wind events, thus underestimating phosphorus and soil dry deposition values.

Response: *The mini-vol sampler and the TSP sampling cassettes on the TWS did not have a design upper cut point for particles. Sampler designs (rain cap for the MVS and hood with inverted cassette for TWS) would discriminate against the collection of very large particles but the exact effect depends on atmospheric turbulence. These large particles contribute disproportionately to the total mass near sources of dust such as roads so the effect could be significant at such monitoring locations but much less significant on the Lake. Because the ambient concentrations were used in both the dry and wet deposition calculations, any undersampling of large particles could affect both dry and wet deposition estimates. Because the phosphorus concentration used in the deposition estimates were assumed rather than measured, the sampler bias would have no impact on phosphorus deposition estimates.*

Action: *Staff enhanced the quality assurance discussion in the report that demonstrated the general equivalency of the various PM measurement methods used during LTADS. Staff better quantified the bias between methods.*

Comment: TWS samplers delivered so little mass available for XRF analysis that even with good sensitivities, relatively few phosphorus data were observed during the entire study.

Response: *The staff chose to use demonstrated continuous aerosol and gas measurement systems instead of intermittent measurements with systems using higher flow rates to address the objectives of LTADS because a full annual record was considered critical to capturing all the potential and variable influences. The TWS has been successfully used in other air quality studies by ARB. Staff was aware that the clean environment would challenge the ability to detect some elements. However, even with the low flow rates in the TWS, the limits of detection claimed by laboratories using XRF were judged adequate to represent phosphorus concentrations if those concentrations were sufficient to result in deposition rates approaching the estimates of deposition discussed in pre-LTADS work. During LTADS, less sensitive laboratory analyses with higher LODs for phosphorus were reported due to matrix effects from sulfur and silica in the samples.*

During LTADS, staff worked with experts to increase the air flow rate slightly without adversely impacting the PM2.5 and PM10 cutpoints. Staff also proposed masking the filters to collect a denser sample for XRF analysis but the logistics of filter preparation, handling, and lab analysis were too complex to implement in a timely and cost-effective manner. The number of phosphorus detects would depend on whether atmospheric dispersion was greater or smaller than in the past, on how large the field blank concentrations would be, and on how strong the local emission sources were. As LTADS turned out, dispersion was slightly better than normal, the field blank values were relatively high, and most of the MVS samples were on piers and buoys that had much lower concentrations than shoreline sites (most of the phosphorus tends to be in soil and thus in the larger aerosols that tend to deposit rapidly). In retrospect, a more sensitive XRF method could have been sought or an alternative continuous monitoring method might have been developed to collect greater filter mass. However, large uncertainties would still remain due to overlap of the weak phosphorus signal with stronger sulfur and silicon signals due to their much larger (~20x and ~200x, respectively) ambient concentrations. With large, highly size-dependent, self absorption correction factors (~5-20x) for particles larger than PM2.5, the uncertainties in measuring phosphorus remain large.

Action: *Staff made crude adjustments (in the PM_large and PM_coarse sizes) for the new theoretical self absorption correction factors and estimated what the ambient phosphorus concentrations might actually be. Based on these analyses, staff raised the phosphorus concentration input to the dry and wet deposition models from 20 ng/m^3 to 40 ng/m^3.*

Comment: LTADS did not make use of prior and concurrent data on fine particles and phosphorus, including prior ARB programs, current TRPA programs, including over 5,000 values phosphorus values at South Lake Tahoe.

Response: *The staff did make use of prior and concurrent data. However, staff chose not to use the current "5,000 P values" because the data were collected with a sampler that is not a federal reference method and has not been thoroughly tested and certified as an equivalent sampling method. Furthermore, the limited amount of data from this sampler to which the ARB has had access compares poorly with data from standard and accepted samplers. Staff also did not directly use measurements made at the SOLA site because during the hours of* *downslope/offshore flow, it is strongly impacted by the emissions from the nearby US Highway 50. Thus, concentrations of pollutants associated with motor vehicles at SOLA are expected to be high compared to concentrations in the greater SLT community and especially compared to concentrations at the lakeshore or on the lake itself.*

Action: *No action taken.*

Comment: LTADS underestimates fine particle phosphorus input to Lake Tahoe because of its inability to see the low ambient levels (few ng/m^3) involved and lack of source information on the automotive and diesel very fine phosphorus emissions. These emissions, plus wood smoke, are trapped under a shallow inversion at night and move out over a goodly fraction of the lake surface, enhancing deposition. This will cause LTADS to underestimate fine particle phosphorus deposition to the lake.

Response: *The staff assumed a fixed phosphorus concentration of 20 ng/m^3 in its initial analysis (draft final report) and also assumed in the dry deposition analysis that 20% of the phosphorus was in the fine fraction. Thus, the initial CARB dry deposition estimate essentially assumed a fine phosphorus concentration of 4 ng/m^3. The dry deposition analysis matched this concentration with seasonally-averaged diurnal wind patterns and assumed an infinitely deep layer of fine particles such that there would be no depletion of material with advection over the Lake (i.e., concentrations at the center of the Lake are the same as concentrations at the shoreline sites). Similarly, the wet deposition estimates, which assume surface concentrations are representative of concentrations through relatively deep layers of the atmosphere, would also tend to overestimate the actual deposition. Unless the actual phosphorus concentrations are higher than a few ng/m^3, it is doubtful that CARB underestimated fine phosphorus deposition to Lake Tahoe.*

Action: *No action occurred as a consequence of this comment. However, in the final dry deposition analysis, staff assumed a constant total phosphorus concentration of 40 ng/m^3 and no depletion of <u>fine</u> particles with distance from the shoreline. Thus, it is now even more doubtful that CARB underestimated fine phosphorus deposition to Lake Tahoe.*

Comment: LTADS upper size cut aerosol capture still does not match the very coarse particle capture in the TRG deposition buckets. With the evidence of LTADS (and elsewhere) that there is significant soil (and thus phosphorus) mass above TSP limits, this will cause LTADS to underestimate lake deposition of very coarse phosphorus and soil.

Response: *The staff acknowledges that the mini-vol and two-week samplers used in LTADS do not capture very large aerosols (> ~30um in diameter). Because of their size, these particles can contribute significantly to the total aerosol mass. Two factors work toward minimizing the potential impact of very large particles not being included in the LTADS estimates. First, such large particles deposit rapidly and most will not transport from their source to the Lake. Second, the optical particle counters used in LTADS indicate a very small number of such particles. Staff also notes that the bucket deposition sampler at Ward Creek Lake Level (aka Wallis Residence or Wallis Tower) likely over-samples large particles due to the presence of trees next to the sampler, and of traffic nearby. Thus, the deposition samplers on the buoys likely* *underestimate and the one at Ward Lake Level likely overestimates the actual dry deposition to the Lake. However, comparison of the limited number of collocated and contemporaneous PM measurements indicates reasonably good PM 2.5 and PM10 measurements by TWS and good TSP measurements by MVS. The data comparison did indicate however that the TSP measurements by TWS could be biased low by about 10%.*

Action: *No action taken.*

Comment: (Section 1.6.5) "…local generation of photochemical smog appears to be the main cause of increased O_3 ….within the basin." 25 years of studies, including some in LTADS, find that almost all O_3 within the basin is transported in from the west slope (see USFS Watershed 2000, ff). This comment is repeated in LTADS elsewhere. I have provided details of the prior arguments below in the appropriate section. The final report must either correct or support this statement, including evaluation of earlier studies…

Response: *This statement was taken directly from a contractor's abstract summarizing the results of their analysis of ozone and nitric acid in the central Sierra Nevada. Staff agrees that this is an inappropriate conclusion and will clarify the statement in the LTADS final report (but is not at liberty to edit the final report of a contractor after it has been accepted). The statement was presumably made because the ozone pattern generated by their analysis of their integrated passive sampling indicated a decline in ozone concentrations with increasing altitude on the western slope of the Sierra Nevada but higher concentrations again in the Tahoe Basin. Such a spatial pattern would be consistent with ozone being locally generated. However, spatial patterns could also be related to the complex dynamics of vertical mixing of air masses over the Sierra crest. Staff's analysis in Chapter 3 of the frequency of ozone concentrations exceeding selected cutpoints at surface monitoring sites provided no indication of the direct transport of polluted air masses. However, occasional indications of ozone downmixing in the surface data, as well as limited ozone measurements aloft, suggest that upwind regions do contribute to enhanced background concentrations of ozone and possibly occasionally to transport aloft of relatively high concentrations that are obviously unrelated to local ozone generation.*

With respect to temporal trends, the LTADS staff does not believe that ozone has been increasing over recent years in the Tahoe Basin – the apparent slight increase in ozone as observed at monitoring sites located within source areas in the basin is likely due to less scavenging of ozone by fresh nitric oxide (i.e., $O_3 + NO \rightarrow O_2 + NO_2$*) emissions as pollution controls reduce the emissions of NO from the motor vehicle fleet.*

Action: *Staff deleted this sentence from the summary in Section 1.6.5 of the LTADS report. Staff included additional data summaries and articulated their findings in other chapters discussing ozone (Ch. 3) and transport (Ch. 6).*

Comment: Whatever the original expectations, LTADS is a very important study, by far the largest air quality study of any kind ever undertaken in the Lake Tahoe basin. As such, it must stand tall as the foundation for all future studies and the basis for all future regulatory actions. Any who picks it up must get a whole overview of the past 35 years of air quality work, since the aerosol ambient concentrations are the key parameter needed to estimate deposition values. But one of the failings of LTADS is, that by using staff and contractors many of whom had little or no experience at Lake Tahoe, much prior useful information was not cited. In fact, the work was probably not even known, as much of the research at Lake Tahoe is in the form of reports as required by the constant regulatory focus, but not the refereed literature.

As a first connection to the past work, a complete bibliography is an excellent beginning. There are two major integrative peer reviewed studies that can provide much of this information – the 1996 USDA Sierra Nevada Ecosystem Project (4 volumes, but the three key papers are “Air Quality in the Sierra Nevada”, “The Case Study of Lake Tahoe”, and “Biological Effects of Air Quality”) plus the USFS Watershed Assessment Study, 2000 (with the key paper Chapter 3, Air Quality). I have included the references from these papers in electronic form as a start to this process.

Response: *LTADS was not intended to be an exhaustive or definitive assessment of air quality and atmospheric deposition in the Tahoe Basin. Rather, it was intended to provide an independent approach and assessment to confirm or refine the direct atmospheric contributions of nutrients and aerosols to Lake Tahoe. Staff did make use of existing literature as appropriate. However, staff did not have the time and resources to perform a detailed critical review of all previous work. Much of the previous efforts are not in peer-reviewed literature. Furthermore, the efforts are not always well-documented and some conclusions appear premature to staff.*

Action: *Staff did not have the resources or need to perform a critical review and summary of all the historical air quality efforts related to the Tahoe Basin. Staff included more details as appropriate and pertinent to support “the interpretations of LTADS, and to help put LTADS into a more global perspective.” Staff included an “Additional Reading” appendix in the final report as a resource to readers interested in learning more. Staff also included electronic links to the Tahoe Integrated Information Management System and other various portals for information on Tahoe (e.g., USGS, TRPA, TRG) on a CARB webpage dedicated to LTADS.*

Specific Comments

Comment: This [Executive Summary] needs to be re-evaluated after important corrections (circa x 3) are made to the predicted TSP phosphorus levels, MDLs, and better information on increased transfer rates from land to water.

Response: *The staff agrees that, given the size of the report, the Executive Summary will be the primary source of information for most readers and interested parties.*

Action: *The staff updated the Executive Summary after the main chapters of the report were finalized to ensure that it conveys the essence, critical findings, and conclusions of the report.*

Comment: (Page v) “the most comprehensive database of atmospheric data ever assembled of the Lake Tahoe region.” This is true only for certain parameters, and is not true for aerosol size and composition, in which LTADS is less than 1% of the existing aerosol data. For example, there is a more complete aerosol profile in (ARB 1979) at 10 sites within the basin, summer and winter. For parameters like soils, these data are still useful. Beginning in 1988, paired sites at Bliss SP (which measures only transported aerosols) and South Lake Tahoe (transported plus local aerosols) are available twice/week. Very limited use was made of these data. Recent TRPA studies included highly size and time resolved samples, including over 5,000 measurements of phosphorus.

Response: *The staff believes that the data collected during LTADS represents the most comprehensive (spatial, temporal, and compositional) set of contemporaneous air quality and meteorological data ever collected in the Tahoe Basin. That is not to say that there are not more detailed components of information available. Staff does not concur with the assumption that the Bliss site only represents transported aerosols. Staff used historical data in characterizing the setting and developing conceptual models. In fact, staff made extensive use of the IMPROVE aerosol data from Bliss and SOLA in its Interim Report. Staff did not make use of the 5000 recent measurements of phosphorus because the data were not released to ARB and the sampling method has not been demonstrated as equivalent to reference methods for collecting aerosol data.*

Action: *Staff revised the statement to say "the largest set of contemporaneous air quality and meteorological data yet assembled to represent conditions in the Lake Tahoe region during four seasons."*

Comment: (Section 1.1 - History) In general, the report would be far more readable if the references were better cited in the text rather than using "passive voice" sentences and a terse reference. In paragraph 2, for example, you could state "Measurements made by Prof. Charles Goldman and the UC Davis Tahoe Research (TRG) Group (http://trg.ucdavis.edu) showed that between the mid 1960s and …" That way readers can go immediately to primary sources.

Response: *The staff agrees that all statements describing past work should be clearly referenced so that readers can examine the original sources and so that those authors are appropriately credited for their contributions to the science. We have endeavored to do so in a standard style. For some readers, the style might not be ideal for readability but it does provide the necessary information to direct readers to original sources. Staff acknowledges that additional statements should be referenced.*

Action: *During the staff's revision of the report, the staff attempted to substantiate all key statements with reference information. However, staff did not invest the time to modify the referencing style throughout the report.*

Comment: (Section 1.1 - History) LTADS would be well served to state at this point that it was designed to handle the most intractable but most important problem in the contribution of air quality to the clarity of Lake Tahoe, atmospheric deposition, but it was building on 35 years of air quality research in the basin, some of which, including the pivotal 1974 "Lake Tahoe Air Quality", was ARB funded.

Response: *The staff does not believe that LTADS, even as initially envisioned before real world constraints entered in, would have answered all the complexities of atmospheric deposition and its impact on water clarity. Instead, staff primarily attempted to provide observation-based estimates of the dry deposition of N, P, and particles appropriate for inputs to the water clarity model and for comparison with the estimates of other contributing sources, such as stream flow and direct runoff.*

Action: *Staff changed the name of this section to "Context" rather than "History" to avoid the temptation to create a historical summary of previous efforts. However, the final version of this section notes the long history of research in the Tahoe Basin.*

Comment: (Section 1.1 - History) Any short term intensive study must be put into the perspective of long term conditions at the site and nearby areas including source and meteorological changes that could reflect LTADS in the long term environment. For example, there was a major forest fire in Oregon in 2002 that lasted for a month and had a significant phosphorus contribution at Lake Tahoe not seen in typical low temperature Tahoe wood smokes, also tracked by non-soil potassium in the 0.34 to 0.56 µm size mode.

Response: *The staff agrees that short-term studies need to be evaluated in the context of longer periods and normalcy. It was for this reason that staff conducted LTADS as a continuous sampling study over an annual period instead of as a series of episodic studies. With this approach, intermittent variations in emissions and meteorological conditions are integrated into the measurements and provide a more stable assessment of annual conditions than would episodic sampling. Staff shares the some concern that the results can, and likely will, still vary from year to year due to changes in the proximity of sources to the sampler and year to year variations in typical weather conditions. The annual weather variations however are likely to be small compared to the other uncertainties in the deposition estimates. The impacts of forest fires are highly variable and difficult to characterize. Staff provided some characterization of the air quality and weather in 2003 compared to historical trends and norms. Of more concern to staff is the uncertainty associated with the potential impacts of forest fires and climatology in the future as philosophies regarding fighting wildfires, conducting prescribed burns, and the global climate change.*

Action: *Staff enhanced the comparisons of conditions in 2003 with historical emissions, air quality, and weather. However, staff acknowledges that the basis of emission trends in the Tahoe Basin is not well-founded and the basis of air quality trends is limited by the paucity of long-term sites with comprehensive monitoring data. Projection of future emissions and especially of weather conditions is fraught with uncertainty and was not attempted.*

Comment: (Section 1.5) "Earlier analytical…." References needed to the 45 years of TRG work. Recent work (Schladow 2004 – see http://trg.ucdavis.edu) shows that fine insoluble particles are a key factor in lake clarity through much of the year.

Response: *The staff's intent was to provide a brief historical narrative of the evolving understanding of pollutants of concern and why staff took a comprehensive look that included* *nitrogen species. As noted, the focus of concern has shifted from algal nutrients (phosphorus and nitrogen), to primarily phosphorus, and, during LTADS, to insoluble particles.*

Action: *Staff included a general reference to TRG's legacy of research in the final report and also included more information about the more recent concern for insoluble particles.*

Comment: (Section 1.6.10) An important discussion, some of which should probably go into the Executive Summary, as it gives a sense of the uncertainties involved.

Response: *The staff agrees that phosphorus measurements are very uncertain, that the uncertainties likely increase with particle diameter, and that reducing uncertainties will take some years as different sources of the uncertainty may be addressed with possible future targeted studies. If XRF is used in the future to measure phosphorus, confidence in results should be based on a more complete theoretical basis for SACFs and hopefully confirmed empirically. It will be necessary to verify the expected effects of different distributions of P within particles and the effects of those distributions on the SACFs. A critical assessment is needed of the actual limitations of any XRF measurement for phosphorus when the sample matrix includes soil particles containing elements with emission spectra that overlap that of phosphorus (e.g., silicon and sulfur).*

Action: *Staff included brief comments on the inherent large uncertainty in phosphorus measurements in the Executive Summary and commented in more detail in Chapter 3 (Data Quality and Summary of Ambient Concentrations).*

Comment: (Section 1.6 - Special Studies) These are important and useful, but all are of relatively short duration and at limited sites. Major extrapolations in space and time are necessary to fit these data into deposition models. These uncertainties are great and must be stressed.

Response: *The purpose of most of the special studies was to address basic assumptions and processes that affect the deposition estimates. The TWS and MVS networks featured year-round monitoring of ambient concentrations to provide a comprehensive integration of conditions at Tahoe in space and time. The extrapolations from the LTADS measurements are actually much less than what has been needed from prior data collection efforts and the appropriateness of the spatial extrapolations can be better assessed by consulting the observations obtained in the special studies.*

Action: *No action was taken as the uncertainties are difficult to quantify. The studies helped to refine understanding of the atmospheric processes and thereby reduce uncertainty in the deposition estimates.*

Comment: (Table 1-3) I deeply regret that the Thunderbird Lodge deposition samplers were deleted.

Response: *The staff agrees that deposition measurements at this site would have been ideal for quantifying deposition at a site primarily impacted by air flow off the Lake and distant from significant local emission sources. However, the available space at the site was limited and the* *proper exposure for deposition measurements was not possible. Staff made repeated efforts to obtain monitoring space at many locations along the east shore, approaching property owners and public agencies. However, appropriate alternative sites with power were generally unavailable. Access and space for monitoring was made available on the Zephyr Cove pier for ARB and DRI equipment and that was greatly appreciated.*

Action: *No action taken.*

Comment: (Section 1.6.4) HNO_3 "…urban areas to the west of Lake Tahoe can not be identified as an important source." This result contradicts airborne results of Carroll et al 1998 on the existence of perched layers of reactive species and ozone above the Sierra, some of which come down at night (also seen at Yosemite 2002)

Response: *Staff supports this statement but wishes to clarify that it refers to potential source contributions to HNO_3 concentrations near the ground surface in the vicinity of the Sierra crest and at lake level. The statement was taken verbatim from the abstract of a contractor's report. The contractors' conclusion was based on ground-level measurements where the sticky nitric acid would not survive transport over the Sierra. The statement is consistent with our understanding of physical processes and with the observations and the conclusion of another LTADS contractor. The statement does not contradict Professor Carroll's observation of perched layers. In fact, staff agrees that nitric acid is likely present aloft, especially in the absence of surfaces and particles. Staff agrees that there is potential for transport of nitric acid aloft to the Tahoe Basin but notes that the physical processes necessary to mix these layers aloft down to ground-level within the Tahoe Basin are not prevalent without substantial dilution and prior deposition.*

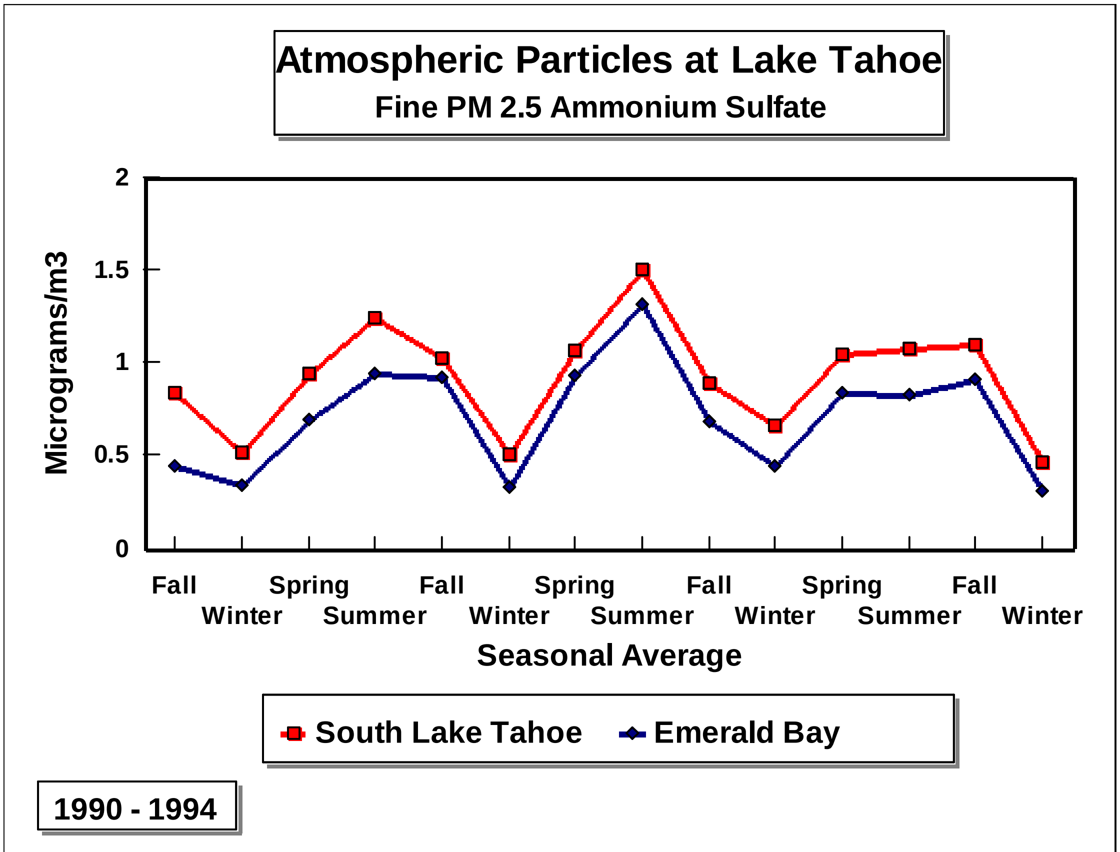
Action: *Staff retained this statement but clarified that it refers to ground level concentrations (and deposition) of HNO_3. Staff acknowledged a potentially greater transport aloft but noted that the potential for subsequent deposition to surfaces is limited due to dilution that would be associated with vertical mixing.*

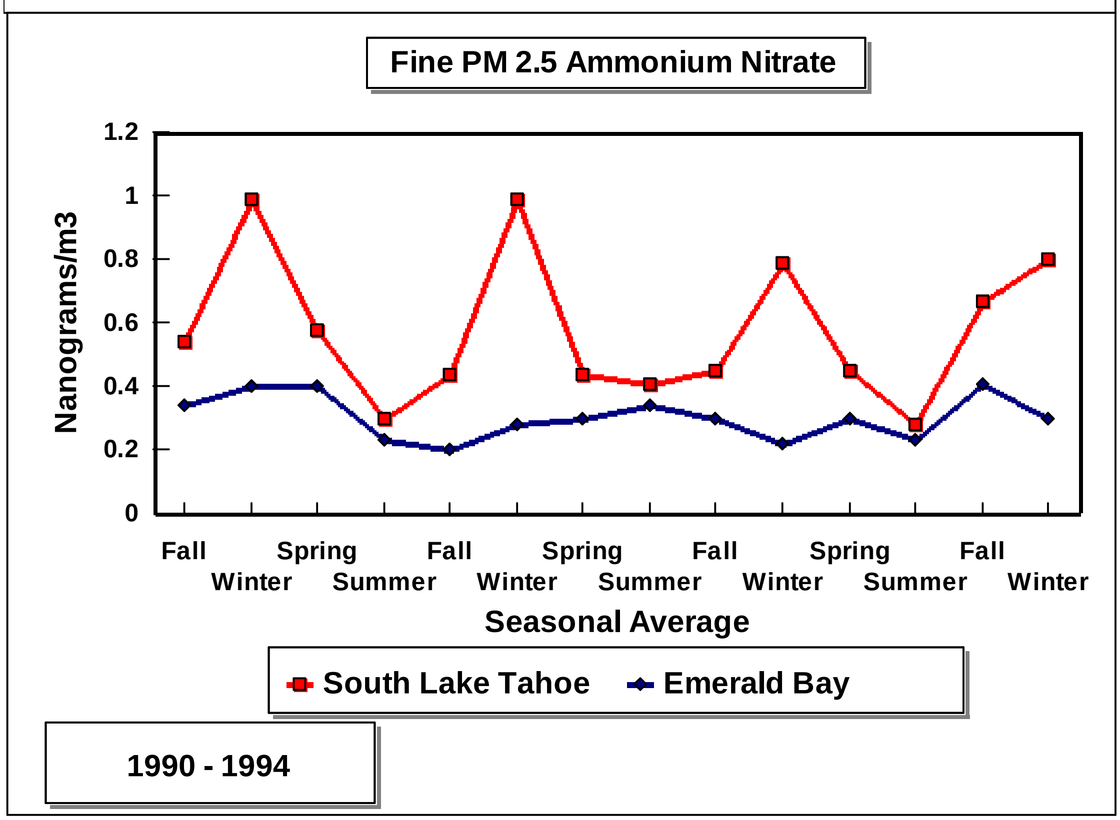
Comment: (Section 1.6.5) "…local generation of photochemical smog appears to be the main cause of increased O_3 …." I hope this was a typographic error, and not the conclusion of this report. 25 years of studies, including some in LTADS, find that almost all O_3 within the basin is transported in from the west slope (see USFS Watershed 2000, ff). The final report must correct this error and include an evaluation of the earlier studies.

Response: *This particular section of the report used abstracts from the contractor reports to provide a summary of the special studies. Unfortunately, the staff did not catch this statement before accepting the final report and repeated it here without sufficient commentary. Staff is not convinced that ozone concentrations have generally increased in the Tahoe Basin in recent years. Because upwind concentrations and emissions of nitrogen oxides have trended downward, staff believes the most likely cause of any slight perceived increase in ozone concentrations at local monitoring sites is due to a decrease in the titration of ozone as local nitric oxide emissions from motor vehicles decrease as new, cleaner vehicles replace older, higher-polluting vehicles in the motor vehicle fleet. Although a decrease in titration by NO can increase ozone concentrations near the NO sources (such as roadways), the broader effect* *across the basin of lower local nitrogen oxide emissions is lower ozone concentrations at downwind locations. Staff believes that the transport of ozone and other reactive species is a complex process. Although it appears that direct transport of ozone concentrations exceeding the 1-hour ambient air quality standards does not occur in the surface layer, it does not mean transport does not occur aloft where concentrations and mixing processes into the Tahoe Basin are poorly known. Staff suspects that the most typical form of transport would be an enhancement of the background ozone concentration and not an episodic event except in infrequent instances.*

Action: *Staff deleted this sentence from the summary in Section 1.6.5 of the LTADS report.*

Comment: As an example of earlier results with direct applicability to LTADS, I present below the USFS Watershed 2000 estimates of transport into the Lake Tahoe basin. These were based on the paired Bliss SP (BLIS) and South Lake Tahoe (SOLA) samplers, 1988- present. This enormous data set designed mostly for visibility studies was able to cleanly resolve transported samples (seen at BLIS) from local plus transported aerosols, (seen at SOLA). BLIS in fact in summer reflects exactly the Desolation Wilderness Area at Lake Aloha (USFS, 1992). The first plot shows ammonium ~~nitrate~~[sulfate], which is always a transported aerosol, and ammonium nitrate, transported in summer, local in winter.





(Note: The ammonium nitrate units should be ug/m^3)

Response: *The staff is hesitant to assume that the Bliss measurements solely represent transport or regional background concentrations. The staff thinks that the Bliss measurements probably reflect a combination of influences that vary in their importance with the seasons: regional background (highest in summer and lowest in winter), basin background (likely highest in summer), and a small local component (likely highest in summer). The SOLA measurements include both the regional and basin background influence but also include a strong local component (likely highest in winter but high year-round). Staff notes that the concentrations at Bliss and Lake Aloha may have been similar during the year of concurrent measurements but, because concentrations are low at both sites, the measurements do not necessarily represent the same air mass. Staff also believes that sulfate indicates an aged aerosol but not necessarily a transported aerosol. Thus, staff believes that the Bliss data represent an upper limit of regional transport because the Bliss data include a basin background component, which is influenced by emissions within the basin.*

Action: *No action taken.*

Comment: It is also easy to add trend data from the Sandy Way site, easy to do considering the availability of the splendid ARB ADAM resource, and meteorology from programs such as www.weatherundergound.com.

To what degree was the LTADS year typical?

Without these types of results, LTADS can not be put into a long term perspective.

Response: *Although ADAM is convenient and informative, the staff does not believe the annual maximum statistics are sufficient for contrasting years and defining trends for annual deposition. Characterizing the meteorological influences that are the major determinants of air quality and deposition would require more complicated metrics and analyses.*

Action: *Staff enhanced the meteorological summaries (but not the air quality summaries) to provide an improved context for the LTADS results.*

Comment: (Section 2.2.3.2 – surface-based inversions) See the surface-based inversions in ARB 1979 (bulk Richardson number data) and USFS Watershed 2002, with pictures. The latter clearly shows the inversion blocking the smoke from a controlled burn from reaching the lake surface.

Response: *The staff has also frequently noted smoke and haze layers above the Lake with little indication of atmospheric mixing that will bring it down to the Lake's surface. On the other hand, staff has observed perched fog and haze layers above the Lake that, during the day, have shrunk from the top rather than being eroded from below. Such an occasion indicates slowly descending air from above (replacing the air being drawn off the Lake and up the mountain slopes) which warms and evaporates the water droplets rather than increased mixing from below the cloud. Staff concurs that surface based inversions are very common in the Tahoe Basin over land especially from late afternoon through early morning, which are the hours when surface air flow is most commonly from the land toward the Lake. Staff included information from the 1979 reference in Chapter 2 (e.g., Figures 2-19 and 2-20). However, staff developed additional information because most temperature profiles measured in the Tahoe Basin are over land and* *these generally must contrast sharply with what occurs over the Lake due to the thermal mass of the water. During night and early morning hours, the cause of most down-slope air flows is a ground surface colder than the air above so that the resultant cooling of the air and increased air density near the surface creates a cold-air drainage flow. But during these hours, the lake surface is almost always warmer than the air so that, as cold drainage flow moves off the land and over the water, it is warmer by the lake surface and experiences thermally-induced mixing. Ground level emissions are mixed vertically through the deepening surface layer and are thus diluted but that layer is not isolated from the surface. However, as you noted regarding the picture of smoke over the Lake, emissions injected into an inversion layer above the mixed surface layer, as may occur due to plume rise from a hot fire, will indeed be isolated from the lake surface unless the temperature structure changes. However, during hours when the lake surface is relatively warm compared to the air, then, at the lower boundary of the visible smoke, there may be some mixing into the surface layer but with sufficient dilution such that no smoke is visible in that layer. Surface-based inversions over the lake occur mainly during afternoons of warmer months as the seasonal water temperature lags the seasonal increase in daytime air temperatures. Staff's desire to understand and define seasonal and diurnal potentials for vertical mixing motivated the measurement of temperature profiles over the South Lake Tahoe Airport by means of the radio acoustic sounder operated in conjunction with the radar wind profiler. Hourly predicted depths of mixing over the land and lake were presented in Chapter 2, based upon the temperatures aloft observed by RASS and the air and water temperatures observed at the lake surface and assumption of rapid and complete heat exchange.*

Action: *No action taken.*

Comment: (Chapter 3) Aerosol data were merged into the USFS Watershed Assessment Lake Tahoe Airshed model (LTAM), a gridded Eulerian model with over 1,000 cells incorporating meteorology, all prior ambient measurements, and traffic data. It might be instructive to compare LTAM model results to LTADS measurements.

Response: *The staff did acquire a copy of the LTAM model. Applications of LTAM were informative for developing conceptual models and theoretical constructs. The results however were not considered definitive and are not easily compared with the ambient measurements from LTADS.*

Action: *No action taken.*

Comment: (Section 3) What data support the upper cut point of the TWS and the Mini-vol?

Response: *Tests were conducted to ensure that the flow rate of the TWS would provide the appropriate particle sizing cuts at 2.5 µm and 10 µm aerodynamic diameter. However, no tests were performed to determine the upper size limit for capturing large particles. The upper limit undoubtedly would depend on the amount of atmospheric turbulence present. The TWS design with its protective hood, downward facing filter, and low flow rate would not capture particles as large as the MVS, which would not capture particles as large as other PM samplers with higher flow rates (30-35 µm). The optical particle counter experiments suggest a limited number* *of particles in the top size bin (>25 µm), especially with moderate distance from emission sources.*

Action: *No action taken.*

Comment: (Section 3) The TWS sampler design was not well coordinated with the proposed DRI XRF analysis protocol. The TWS collected 26 m^3 of air but (correct me if I am wrong) spread the sample over a 47 mm filter. This results in an areal density of circa 13 cm^2/26 m^3, or 0.50 cm^2/m^3. This is the number that the XRF data (in ng/cm^2) must be multiplied by to get ambient concentrations in ng/m^3.

Response: *This comment is closely related to a comment and response on page J-5. The staff was aware that the XRF results would be pushed to their limits for some species (e.g., P) and investigated masking the filters to increase the sample density. However, doing so turned out to not be compatible with the laboratory procedures. The staff pursued additional analytical methods (longer exposure, ICPMS, s-XRF) in its efforts to increase the frequency of P detections. The s-XRF method, which was used on 70 ambient samples that were selected to provide a good spatial and temporal distribution, provided a reasonable number of detections and insights that the staff used in estimating the P concentrations during LTADS. Staff acknowledges the design flaw with respect to measuring a critical nutrient to Lake Tahoe, which was ameliorated but not overcome by adjustments during LTADS. Furthermore, the discovery (after the field study and lab analysis) that large self absorption correction factors are needed for measuring P in particles larger than 2.5 µm creates sufficient uncertainty in the actual ambient P concentrations that only "ballpark" estimates of P concentrations are appropriate.*

Action: *No action taken.*

Comment: (Figure 3.3) The 50% difference between the sum of species and TSP mass for the entire LTADS data set, but best seen at SOLA and Sandy Way, may represent an inadequate correction for self absorption in the TSP XRF data. Note that because other species contribute to TSP (OC,…), and some soil species are heavier and have smaller corrections, (Ca…Fr…), the actual XRF corrections for light elements will be still larger. This especially affects the phosphorus data (see above) that may be low by a factor as much as 3.

Response: *The staff believes some of the mass difference between sum of species and gravimetric analysis is due to water associated with the particles. Given the theoretical analysis of self absorption by particle size, staff is very concerned about the ability of any XRF method to reliably measure metals in particles larger than 2.5 µm.*

Action: *No action taken.*

Comment: (Section 3.2.4) This is a very important section, but would benefit by matching with meteorology and using CalLine 4 type modeling.

Response: *The staff agrees that additional analyses combining ambient concentrations with meteorological situations could yield valuable insights into the nature of pollutant sources.*

Simple modeling applications would also help to confirm and quantify some of the processes at work.

Action: *No action taken. Additional data analysis was already recommended in the report.*

Comment: (Section 3.2.6 - Dust experiments) The new work (Schladow et al 2004) showing the importance of fine soils in lake clarity make dust data much more important.

Response: *The staff agrees that the PM deposition estimates have enhanced importance with respect to the clarity of Lake Tahoe. In particular, staff should revise (downward) the initial (conservative) PM deposition estimate to account for the effects of particle solubility and possible sedimentation of large particles (by own mass and added mass associated with algae attaching to it).*

Action: *Staff incorporated more discussion of the soluble fraction of the PM into the final report. The draft report noted an approximation of the soluble fraction in PM (25%) but the staff articulated further in the final report the amount that the dry and wet deposition estimates would need to be discounted to account for particle solubility in water.*

Comment: (Figure 3-26) Reliance on the data of one day (March 12, 2004) on weak night winds without a lot of supporting data on weather, road conditions, and other factors are not very useful. We saw at the same site in different conditions massive transport from the road over the lake (ARB 1979).

Response: *The staff had hoped to conduct more particle count experiments and data analyses than they had time to do. Staff acknowledges the limited data base but also notes that the data were intentionally collected under rather typical conditions. Staff also notes that sanding and sweeping practices are different now compared to 25 years ago.*

Action: *Staff provided more documentation of the conditions when the experiment was conducted.*

Comment: (Section 3.2.8) How do these data compare to TRPA averages? Since there are clearly factors of 2 year to year, these directly affect the uncertainties in any deposition result based on aerosols.

Response: *The staff acknowledges that meteorological variations from year-to-year can cause large variations in ambient concentrations. The year-to-year variations in concentration also depend on the specific pollutant. Without a long data record for many of the pollutants at the TWS sites, it is difficult to quantify the typical range in ambient seasonal concentrations. For this reason, staff tended to use assumptions that would keep the range of possible deposition outcomes wide.*

Although there is not sufficient data to support estimates of year-to-year variability some insight can be gained from considering the seasonal variability in concentrations and deposition.

Section 4.7.1 presents the estimates of seasonal and annual deposition for each quadrant and for the Lake as a whole. Tables 4-8 through 4-10 and Figures 4-28 and 4-29 provide details. For both total nitrogen and PM mass, concentrations and dry deposition rates are lowest in the spring. Dry deposition rates for nitrogen are relatively high in summer and fall. Dry deposition rates for PM mass are relatively high in summer and winter. Clearly, the emissions activity, snow cover, and meteorological variations will result in significant differences between seasons for both concentrations and deposition at a high altitude location like Tahoe with very distinct seasons. These seasonal variations should be far greater than any year-to-year variations.

Action: *No action taken as the spatial and temporal records of pollutant concentrations in the Tahoe Basin are limited.*

Comment: (Section 4.0 – Dry Atmospheric Deposition) This section needs work. The BAM data and earlier (ARB 1979, USFS 2000) work show that the maximum of the aerosol data occurs just as the winds are making the transition from weak downslope at night to strong upslope in daytime. The 6 to 8 PM period occurs in this latter condition. Note also that there is clearly aerosol mass even beyond 30 µm, data that are "iffy" with optical particle counters.

Response: *The staff assumes that Professor Cahill meant 6-8 AM rather than PM. The staff acknowledges the strong combined effect of fresh emissions and meteorology. It was for this reason staff used the BAM data to resolve seasonal PM mass concentrations on an hourly basis and used the resulting concentrations along with the observed hourly meteorology in calculating hourly dry deposition rates. The particle counter should have been able to "see" the large particles even better than the other samplers. Unlike samplers designed for long-term deployments, the inlet of the optical particle counter was upward facing without rain cap or other protective obstruction. In comparison to the fine and coarse particles, the steeper concentration gradient observed for larger particles downwind of emission sources suggests that few of the largest particles measured at SOLA remain in the surface layer until reaching the Lake. In addition, impaction onto surfaces will preferentially remove the largest particles. Thus, staff suspects that the very large particles seldom cover the distance from their emission sources to the lake due to the presence of buildings and trees that assist in the depletion and deposition of large particles.*

Action: *No action was taken in direct response to this comment.*

Comment: (Figure 6-1) Arrow #5 has to face the inversion problem.

Response: *The staff noted the inversion complication in various locations of the report and agrees that the figure should also indicate this barrier to transport.*

Action: *Staff modified the figure to indicate the frequent presence of the semi-permanent temperature inversion at about 10,000 to 11,000 feet ASL and also the ground-level temperature inversions due to nocturnal cooling and cold air drainage off the mountain slopes.*

Comment: (Section 6.1) We do not concur with Carroll et al, 1998, "high concentrations of pollutants do not appear to reach the high altitude slopes of the

Sierra" as shown by the results of Gertler et al, showing that for many parameters, including nitrogen, maxima rates of deposition are reached at the Sierra crest. We have long agreed that a sharp fall off of pollutants occurs just east of the crest (ARB 1978 ff).

Using the decade of TRPA BLIS to SOLA comparisons, (above) we can estimate the local fraction of aerosols, summer and winter (USFS Watershed Assessment 2000).

Response: *The 1998 aircraft study focused on ozone and NOy measurements during periods likely to be conducive to effective movement of polluted air from the Central Valley into the Sierra Nevada. Observations in the vertical sampling plane at the high elevation transect no longer indicated a high concentration ozone plume from Sacramento but rather an enhanced background concentration associated with increased mixing of the air mass. The NOy concentrations were low, consistent with limited emission sources, pollutant reactions, and good atmospheric mixing. Staff believes that normal atmospheric processes will dilute and deposit many of the pollutants transported in the surface layer. Ambient ozone monitoring programs in the central sierra Nevada indicate that ozone concentrations typically peak west of the Sierra crest and the effective transport of an air mass with high ozone concentrations infrequently extends further up the slope than about the 6000' elevation. Meteorological conditions that could move a polluted air mass to the Sierra crest in one day are typically conducive to the dispersion and deposition of the pollutants. Staff concurs that the bulk of the deposition of pollutants from the Central Valley will occur on the western slopes of the Sierra Nevada and decline rapidly on the lee (eastern) side of the Sierra. As noted earlier, staff believes that characterizing air quality at the Bliss sampling site as solely representing the impact of pollution transported into the Tahoe Basin is a simplifying assumption that ignores natural, regional, and the limited local emissions. Thus, the Bliss concentrations only represent an upper bound of the impact of material transported into the Basin and the difference in SOLA-Bliss concentrations, rather than quantifying the impact of local (SLT) emissions on air quality, only represents a lower bound of the impact of emissions within the Tahoe Basin.*

Action: *No action taken in response to this comment.*

Comment: (Section 7.2.1) The reliance on a Sand Harbor, NV, site to characterize dust transport from roadways must be supported, as that location is characteristic of no other site in the basin (very narrow coastal plain, precipitous eastern mountain wall, low traffic volumes, little heavy truck traffic, unusual soil, little sanding and salting operations in winter, ,…). There are prior ARB data from other sites including South Lake Tahoe (ARB 1979) that show far more dispersion of road dust at that site, both in concentration and distance. This can be caused by a number of factors:

1. The z_o surface roughness parameter is high at SOLA because of the broken cover of roads and trees. This will loft and disperse pollutants,
2. Heavy truck traffic itself mixes the air to heights roughly 1.5 times the truck's height.
3. Heavier traffic and low wind velocities cause effluent heating in the waste heat of cars and trucks, which will maximize at dawn and dusk during rush hours. (ARB 1974 Freeway Study)

This is a key point because if one assumes a rapid removal of coarse particles, lake deposition values drop very sharply.

Response: *The road dust measurements at Sand Harbor were not directly a part of the LTADS project and these data were not used for calculation of dust transport or for prediction of concentrations or deposition. These data were collected in concert with another DRI contract with the National Cooperative Highway Research Program. By making the measurements at this site concurrent with the flux tower study, our contractor essentially gained a calibration point for assessing the road dust measurements that were made around the Lake for LTADS.*

Staff agrees that more dispersion might occur in the immediate vicinity of the roadway near SOLA compared to Sand Harbor due to heavier traffic volumes and more trucks in South Lake Tahoe than Sand Harbor. The mechanical mixing associated with vehicles is expected to dominate the initial mixing and temperature would be a relatively minor factor. However, the most significant factor for deposition affecting the Lake is the prevailing wind direction. The drainage winds in the area of SOLA are likely stronger and more persistent than at Sand Harbor due to SOLA's location on the south shore, near steep terrain with a larger drainage area and a more northerly-facing mountain slope which would be expected to provide more hours of negative net radiation and drainage winds. The staff acknowledges that PM deposition estimates depend on the concentration of particles (especially larger particles), the depth of mixing (mixing height), and the residence time of large particles. Trees between the roads and the Lake will tend to enhance both mixing depth and particle deposition.

Action: *Staff included PM deposition estimates by particle size (i.e., PM_fine, PM_coarse, and PM_large) in the final report to provide more insight into the potential impact on the total atmospheric PM deposition if other assumptions were to be made regarding the deposition rate of large particles. Staff also allowed some depletion of larger particles in its final estimates.*

Comment: (Section 7.2.1) A second point is that the dust study "no detected phosphorus above uncertainties". Thus, we must assume that the estimates of aerosol emissions of phosphorus are based on the phosphorus content of soils. In fact, we have found at numerous sites that the phosphorus content of road derived aerosols is greater than that of the original soil matrix …. Thus, the phosphorus estimates will be about a factor of 2 too low based on road soils.

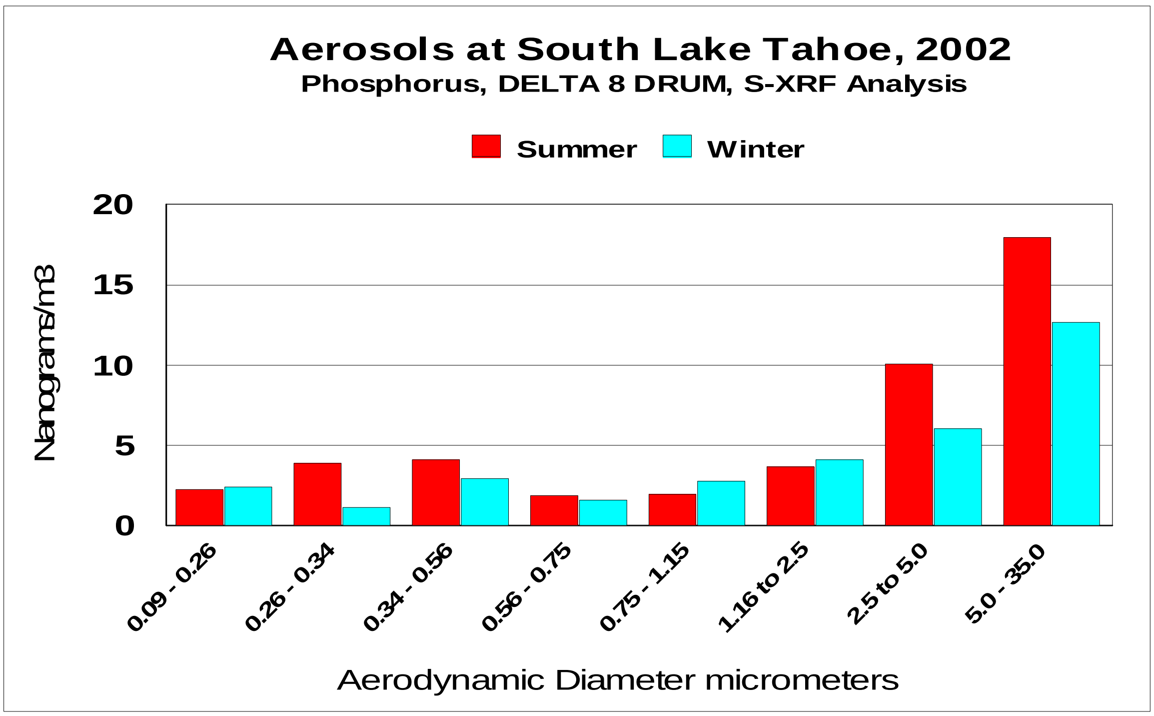
Response: *The staff assumed a spatially and temporally constant P concentration of 20 ng/m^3 for the initial deposition estimate based on the number of samples with non-detects, measurement uncertainties, and more precise analysis of phosphorus at the ALS lab. Based on peer-review comments concerning variable size-dependent Self Absorption Correction Factors (SACF) and other corrections, staff increased the estimate of P concentration to 40 ng/m^3 for calculating deposition estimates in the final report. As in the draft report, this concentration was assumed constant for all locations and times. Other analyses indicate that 40 ng/m^3 is a reasonable estimate of the average P concentration in the Tahoe Basin.*

Staff suspects the reason that the fractions of P in ambient soil measurements can be twice that of "clean" soil may be due to the contamination of the soil with the presence of animal and plant detritus in large size modes as well as deposition of P in small size modes related to the combustion of fuel and oil in motor vehicles.

Action: *For the calculation of estimated deposition rates in the revised final report, staff increased their estimate of annual average phosphorus concentrations in the Tahoe Basin from 20 ng/m^3 to 40 ng/m^3 based on the new information provided from multiple analytical approaches. Staff has described the supporting information and analyses in the revised final report.*

Comment: (Section 7.2.2.2) “One the other hand, phosphorus concentrations were so low as to be below the limits of detection.” Recent data from DRI (Gertler et al, 2002, Zielenska et al, 2002) and the American Lung Association (Cahill et al, 2004) have tied truck and car exhaust to phosphorus emissions via the zinc thiophosphate in lubricating oil plus sulfur in diesel fuel.

…Finally, even in the average data, summer and winter, very fine phosphorus is seen. The particles from about 0.34 to 0.75 µm are from wood smoke, which in the summer was dominated by the Oregon fires …



Response: *The staff acknowledges these combustion sources of phosphorus but notes that most of these emissions are in particles smaller than 1 µm. These particles comprise only a small fraction of total mass and have low deposition velocities. Also the newly recommended SACFs are smaller for the finer particles. Thus, these combustion emissions would have only a small impact on estimates of total P deposition. Staff also notes that although the P fraction is small for dried wood, it can be substantial when live vegetation burns (e.g., wildfires). Staff used the Two-Week-Sampler to ensure temporal integration during LTADS so that the impact of any intermittent emissions from fires (e.g., Gondola Fire) would be represented in the record of observed concentrations.*

Action: *No action taken.*

Comment: (Section 7.2.3) The phosphorus results of Turn et al 1997 in JGR give the phosphorus content of smoke. This should have been at least cited.

Response: *The staff acknowledges that the number of samples for estimating the P fractions of various PM sources is limited; furthermore, the results often exhibit substantial variability and have not always received a thorough review. The results of Turn et al., at least for P, are comparable to the factors used in ARB's emission inventory. As such, the Turn results were not singled out from the other studies that provide input to the development of emission factors. Lastly, the Turn results for P like other results are "soft" because the measurements are frequently less than the measurement uncertainty and also do not include the potentially large, size-dependent self absorption correction factors for P that was identified during the peer review of this report.*

Action: *Staff included a table of the ARB's official P emission factors for PM sources and will include the Turn results for phosphorus in a footnote.*

Comment: (Section 7.3 – Analysis of historical aerosol data) Good work. It should be used to say to what extent the LTADS year was typical. These results would have been most useful much earlier in the report.

Response: *The staff did not have the 2003 IMPROVE data nor the time to analyze it at the time of the draft report. Other staff commitments prevented a 2003 update and comparison with other years would be available for the final report. This section is actually from the LTADS Interim Report. Because the material is so comprehensive (topics and details), staff chose to present its summary in Chapter 7 because it primarily addresses PM sources and also to report the complete material in Appendix B.*

Action: *As appropriate in earlier sections of the report, staff alluded to the IMPROVE data and its implications.*

Comment: (Section 8.1) The Conclusions should be changed to match the revised Executive Summary, and reflect any corrections needed (viz, phosphorus ambient concentrations deposition rates, …).

Response: *The staff intends to revise the report to reflect the most recent understanding of atmospheric deposition to Lake Tahoe.*

Action: *The staff revised the Conclusions and Executive Summary to reflect significant modifications resulting from addressing the concerns of the peer reviewers.*

Comment: (Section 8.1 - Particulate matter) In winter, yes, roadways are dominant. In spring, roughly 50% of the soil is transported in (USFS Watershed 2000), and in summer, many sources are operational, not just roadways (TRPA 2002).

Response: *The staff maintains its position that the Bliss data do not solely represent the impact of transport from outside the Tahoe Basin. There is undoubtedly a basin background* *component as well as a weak and variable local component associated with traffic and smoke in the general area.*

Action: *No action taken.*

Comment: (Section 8.3) Phosphorus. As I mentioned before, the phosphorus detection problem was compounded by a sampler design problem in the TWS. Our DELTA Group, UC Davis S-XRF is at present the most sensitive non-destructive instrument in the world, which is why we developed it from 1992 to present and use it on DRUM and (occasionally) IMPROVE samples. It could be applied with great effect to the hundreds of archived PM_{10} samples from Lake Tahoe, 1978 – 2004.

Response: *The staff thanks you for your offer. Staff recommends that additional XRF analyses for phosphorus, especially any analyses of samples of coarse or large particles should await experimental verification of the potentially large size-dependent self absorption correction factors. Work in progress and further refinement of the SACFs should also better define real-world LODs for phosphorus and other compounds. These efforts could cause staff to further refine their current best estimate of phosphorus concentrations (40 ng/m^3) in the Basin and that was the basis for the LTADS estimate of phosphorus deposition in the revised final report.*

Action: *No action taken.*

Comment: (Section 8.4.2) "…ozone transport may occasionally occur." I believe abundant data show that essentially all the ozone at Lake Tahoe is transported, as shown by

a. O_3 peaks that occur after the sun has set,
b. uniform O_3 profiles across the basin when sources are localized,
c. the short residence time in the basin for reactions to occur,
d. the inversion barrier that keeps precursors at ground level,
e. 10 year temporal profiles that were slightly rising in response to increasing western Sierra (Highway 50) sources versus decreasing sources within the basin (Popejoy, ARB 1993).

Response: *The staff acknowledges the different interpretations people have of the word "transport". Meteorological processes aloft in the Tahoe Basin are complex and incompletely understood. Staff acknowledges that most of the ozone in the Basin is associated with background concentrations created by global and regional transport and also stratospheric intrusions. Although evidentiary point e) is a reasonable hypothesis, Research Division staff has analyzed the data and believes that the slightly increasing ozone trend is more likely due to less titration of ozone within the Basin as a consequence of the decreasing NO emissions within the Basin ($O_3 + NO \Rightarrow O_2 + NO_2$).*

Action: *The staff revised and clarified the discussion.*

Comment: (Section 8.4.3) Either prove to me that ozone is regularly made in the basin or delete this comment.

Response: *The staff concurs that the statement is inappropriate and likely in error.*

Action: *Staff revised the stated implication to be more consistent with the available data.*

Summary of the Major Comments Received from Professor Keith Stolzenbach with CARB Staff Responses

General Comments

Comment: This report focuses entirely on direct deposition to the water surface. In a study of atmospheric deposition in the LA area, we found that the atmospheric loading to streams and water bodies was more influenced by deposition on the watershed that is then washed off than by direct deposition on the water surface. Of course, Lake Tahoe has a large surface area relative to its watershed, so this may be less so there.

Response: *The focus of LTADS was direct atmospheric deposition to the Lake. Another group estimated the nutrient and particulate matter received from runoff and other avenues of input to the lake. The runoff estimates include any atmospheric deposition to land or vegetation that was not sequestered by soil or vegetation. The Tahoe Basin is unique in that the lake occupies a large fraction (~60%) of the watershed. Sequestration of any atmospheric deposition over land is expected to be much greater than in the Los Angeles area because much of the watershed is forested and thus expected to facilitate percolation of precipitation into the soil and uptake by vegetation. Because the soils in the Tahoe Basin are relatively sterile, much of the atmospheric deposition of nutrients to the land surfaces will be sequestered before being able to migrate to the lake.*

Action: *The staff increased the emphasis in the Introduction Section of the revised report that the focus of LTADS was on direct deposition but also briefly expanded the discussion of indirect deposition.*

Comment: In my judgment, the precision of deposition estimates, whether on the basis of direct measurements or theoretical calculations, can not be considered to be better than about ±30-50%. This uncertainty is the result of limitations in measurements of temporal and spatial distribution of the atmospheric concentrations of the substances of interest as well as inadequacies of the theoretical formulations for predicting deposition.

Response: *The staff acknowledges that measurements have inherent field and lab limitations influencing precision and that models, even with limited assumptions, have significant uncertainties and variations in the underlying inputs. Staff concurs that the precision of the deposition estimates is likely not much better than ±50%. To help the reader recognize the limitations of the deposition estimates, staff has typically rounded the numbers to two significant figures. Even so, this implies much more precision in the phosphorus estimates than is the case.*

Action: *The staff included more caveats about the precision of the deposition estimates in the final report.*

Comment: This study deliberately did not make much use of surrogate surfaces, correctly noting the issues relating to realistic collection of small particles. Here in LA we found that relatively simple surrogate surfaces gave us excellent estimates of dry deposition of particulates averaged over a season, largely because most of the deposition was by large particles that are collected the most accurately by such surfaces. Given the results of the size dependent deposition reported in Chapter 4 (see below for my comments), such surfaces might have been very useful in LTADS.

Response: *The staff agrees that surrogate surfaces will have less problems representing deposition of the larger particles compared to fine particles. Especially near sources the larger particles may contribute a significant portion of the mass and deposition. Surrogate surfaces with aerodynamic properties similar to the natural surface of interest are expected to provide more representative measurements. Staff arranged for use of a surrogate surface sampler which was aerodynamic and incorporated a water surface. However, difficulties in the logistics of deployment and field maintenance prevented collection of data with that sampler. In addition, staff purchased additional wet/dry deposition bucket samplers for use in LTADS although they are not aerodynamically similar to the lake surface. Unfortunately, significant siting, operational, and staffing constraints prevented their deployment except for use in the comparison study of dry deposition surrogate surface samplers. Although staff has concerns about interpretation of dry deposition using the results from bucket samplers, staff believes that the existing wet deposition measurements associated with the Tahoe Research Group (wet bucket and bulk samplers during wet periods) and the National Atmospheric Deposition Program at Sagehen provide better estimates of the wet deposition than the ARB analysis. In particular, staff expects that the on-lake measurements provide more relevant data, because, compared to the land-based buckets, they are not as subject to local influences (e.g., emissions, growth of nearby vegetation) specific to the individual sites. The logistics of collecting the on-lake surrogate surface measurements are non-trivial and provide highly relevant data, which should be utilized to the extent possible. The on-lake data should be utilized to the extent possible to characterize deposition during wet periods.*

The ARB wet deposition analysis was needed to confirm the ability of the ARB model using ambient concentrations to crudely replicate the surrogate surface results for nitrogen and phosphorus. By using the same basic assumptions, the staff could then apply the model to generate wet deposition estimates for particulate matter which are not measured with the surrogate surface samplers. The wet and dry atmospheric deposition estimates for PM could then be incorporated into the water clarity model to assess the impact of atmospheric PM on Tahoe's water clarity.

Action: *The staff took no action in response to this comment other than clarifying existing discussions in the report.*

Specific Comments

Comment: On page 1-12 there is a discussion of the implications of the particle size distribution in water. In considering this issue, it should be recognized that once particles are deposited on the lake surface there are a variety of processes, including physical and biological aggregation, dissolution, and chemical transformation, that will make the particle size distribution in water very different from that in air.

Response: *The staff concurs.*

Action: *The staff enhanced the discussion of this topic in the final report.*

Comment: Equation 4.16, representing deposition by Brownian motion and inertial impaction is quite often used, but is theoretically applicable to smooth surfaces only. This limitation is also ignored throughout the literature. There are formulations applicable to rough surfaces, and some papers correctly use them. I have not been too concerned about the error introduced into the deposition calculation because I suspect that aerosols in the size range affected by these assumptions do not contribute much to the total deposition over water.

Response: *The surface of Lake Tahoe is aerodynamically smooth during most hours because of the characteristically low wind speeds.*

Action: *The staff added text in the revised final report to clarify that the formulation used is strictly applicable only to aerodynamically smooth surfaces. Staff also predicted, based on the observed wind speeds, the fraction of time that the lake surface is aerodynamically smooth, aerodynamically rough, or in transition between smooth and rough.*

Comment: The uncertainty in the friction velocity calculation over rougher land surfaces (caused by the low measurement height relative to the roughness height) shows up dramatically in the inertial impaction term because the Stokes Number is proportional to the square of the friction velocity. In the LTADS calculations this would be important only in the near-shore region where the land-based friction velocity is used.

Response: *The staff concurs that the prediction of deposition velocity in the near shore zone would be uncertain (for the periods of offshore wind direction). The friction velocity in the near-shore zone during hours of offshore wind direction is uncertain because one cannot assume a logarithmic wind profile at the heights of the wind measurements. Further, predictions of the aerodynamic resistance and the deposition velocities based on the observed winds would be very sensitive to the (likely poorly) estimated friction velocity. Therefore, for the near-shore zone during periods of offshore winds, the staff assumed aerodynamic resistance values (the capping values for $1/R_a$) such that resulting deposition velocity estimates would be sufficiently large to prevent underestimation of atmospheric deposition.*

Action: *The staff attempted to clarify these points in the final report.*

Comment: The measurements made near roadways are entirely consistent with our comparable measurements here in LA in terms of the distance downwind before the particle concentration is lowered by dispersion. This small spatial scale fits in well with the goal of understanding the spatial patterns of deposition on the lake. I would be cautious about the statement on page 4-53 that much of the material resuspended from the road deposits within a short distance. There is a substantial mass of smaller particles that do not deposit significantly and whose concentration is reduced primarily by dispersive dilution as discussed in the report.

Response: *This statement served primarily as an introduction to the observations made with the particle counters. The staff agrees that it requires qualification. Although qualification was* *provided elsewhere in the report, the discussion here would be tangential and distracting. Transmittal of the main ideas is better served by simply deleting this statement.*

Action: *This statement was replaced with a more appropriate and descriptive statement - "…as expected, observations show that concentrations are depleted and size distributions change significantly within a short distance downwind."*

Comment: The temporal variations in constituent concentration and calculated deposition are interesting and are obviously dominant features at this site, largely because of the diurnal pattern of upslope and downslope air flows. However, it should be noted that the empirical methodologies used to calculate the deposition rates are based on expressions calibrated to transports averaged over at least a day. There have been few if any studies of transports at an hourly time scale. For this reason, the hourly variations in calculated deposition should be considered semi-quantitative at best.

Response: *The Staff agrees that the hourly variations in calculated deposition should be considered semi-quantitative at best, both for the reason cited and because of other sources of uncertainty in concentrations and deposition velocities. Although studies of particle deposition having high temporal resolution are relatively recent compared to those for many gases, e.g., SO_2 or O_3 there is a substantial and growing body of literature quantifying particle deposition rates based on eddy covariance measurements and resulting in flux estimates with hourly (or better) temporal resolution.*

Action: *No action taken.*

Comment: I note in Figure 4-29 that the seasonal variation in PM and N is modest. This is consistent with our results in the urban areas of LA. We interpret this as partially the result of significant resuspension (by traffic and wind) of dust and associated contaminants that tend to homogenize the region and modulate both spatial and temporal gradients.

Response: *The staff notes that the seasonal variations in nitrogen concentrations at Lake Tahoe are modest and that the patterns of relative concentrations by season are similar at the different sites (Figure 4-28). For PM (Figure 4-29), the seasonal variations in concentration are similar, except during winter. In winter, PM mass concentrations were relatively high at the south shore site and low at the north shore site. The location of the sites and the patterns of emissions activity may provide some explanation for the differences. The north shore site (Lake Forest) is located east of Tahoe City and just south of Highway 89. It is relatively isolated from local sources other than Highway 89 (so this site is most impacted during periods of downslope and offshore wind direction with substantial traffic). In contrast, the south shore site (Sandy Way) is located within South Lake Tahoe and is surrounded by local sources so that it can be impacted during any wind direction. It is downwind of Highway 50 during onshore flow.*

Action: *No action taken.*

Comment: As in most regions, the emissions estimates, based on silt loadings and traffic densities, indicate that road dust is the major source of PM. I do not have any firm evidence to contradict this result, but our experience in LA has led us to believe that the road dust estimates may be high and windblown dust estimates may be low. One indirect piece of evidence for this is that, if the road dust emissions were as high as estimated, there is no identified mechanism for replenishment of the silt loading on the road itself. Roads can be a local source of the largest particles that are hardest to resuspend by wind, but, as shown in this report, the region of influence of these particles is confined to a 100 meters or less from the source. What we need to understand better are the true sources of dust (we think it is mostly crustal material resuspended by wind) and the dynamics of the cycle of resuspension and deposition.

Response: *Thank you for this observation. As you know, there are commonalities and some important differences between the urban areas of Tahoe and Los Angeles that would affect the loading of roadways with crustal material. First, many Tahoe roads are intentionally sanded to improve traction in winter. Second, unpaved shoulders and parking on unpaved surfaces is fairly common at Tahoe. Thus, we expect track out of mud from unpaved shoulders and unpaved areas is a more substantial contributor to loading of the Tahoe roadways. Dust raised by vehicle travel on unpaved shoulders or roads may also simply redeposit on paved roads (as well as other areas). However, it will be resuspended on the roads by traffic whereas resuspension on vegetated areas is less likely. The non-urban areas of Lake Tahoe would provide a greater contrast to LA. Much of the area around the Lake has only light traffic with the exception of the main highway that encircles the Lake at varying distances from the shore. In much of the non-urban area, the highway is bordered by forest or other vegetation that would limit resuspension.*

Action: *No action taken.*

Comment: The review of aerosol data is a valuable synthesis of data, particularly with regard to the potential importance of long range transport. Our studies of deposition in Los Angeles have implicated inter-regional transport even of fairly coarse particles as a result of resuspension and transport by wind and traffic.

Response: *The phenomenon described is thought by staff to be primarily important in areas dominated by hard driving surfaces and vehicle traffic. In some urban areas of Tahoe, it could be a factor, but most of the urban areas are interspersed with vegetated, non-paved areas, not subject to vehicle traffic. The vegetated areas are expected to retain deposited particles and be subject to relatively little resuspension. The available observations of concentration gradients suggest that concentrations associated with roads in the Tahoe environment decline with distance downwind before reaching the Lake and are not replenished by resuspension by wind. The observations were made primarily during low speed drainage flows and so suspension or resuspension of geological dust could be a factor during high wind events.*

Action: *No action taken.*

Summary of the Major Comments Received from Professor Gail Tonnesen with CARB Staff Responses

General Comments

Comment: The goal of the LTADS study was to assess the relative contributions of local and transported nutrients nitrogen (N) and phosphorus (P) and fine particulates (PM) directly deposited into Lake Tahoe. Runoff from land areas in the surrounding watershed is also a possibly significant contributor to nutrients and PM into the lake, but these sources will be assessed in a separate watershed analysis to be completed by the Lahontan Regional Water Quality Control Board and RWQCB contractors. Thus, the focus of this study is atmospheric emissions, chemical transformations, transport and direct deposition into the lake. The question of the importance of direct deposition into the lake versus run off from surrounding areas cannot be addressed until the separate watershed analysis is completed. However, it would be useful to include in the summary of this report a description of the research plan and schedule for the water shed analysis.

Response: *The staff does not have detailed knowledge of the watershed research plan and analysis and a comprehensive summary of the direct and indirect atmospheric contribution is beyond the scope and resources available for the LTADS report. If results from the recent research are released before this report is finalized, staff will include the updated inputs to the Lake from the various pathways and a brief discussion contrasting the relative contribution from each pathway with earlier estimates and also contrasting the relative amounts of direct and indirect atmospheric deposition. The technical TMDL being developed by the LRWQCB will include the requested information.*

Action: *No action taken because summaries from all ancillary efforts were not available before the report was finalized.*

Comment: The contribution from runoff will include a component from atmospheric deposition and components from other land use activities, such as fertilizer use, erosion, etc. It is likely that the assessment of nutrient deposition to land and subsequent run off to the lake will make use of many of the same measurements and modeling described in this report. It would be useful to describe in this report the deposition of nutrients both directly into the lake and to land within the Lake Tahoe basin. It might also be useful to compare and contrast the Lake Tahoe Basin with the previous studies of nutrient loading in the Chesapeake Bay watershed.

Response: *The staff has not seen the watershed report and cannot provide a detailed discussion. Because the staff does not have access to the final input numbers from the watershed modeling, the direct and indirect atmospheric inputs to Lake Tahoe cannot be contrasted yet. Such a comparison is more appropriate in the TMDL than in this atmospheric deposition report. Because the Lake Tahoe Basin and Chesapeake Bay are drastically different settings and staff* *has limited time and resources for finalizing this report, staff will not contrast the Tahoe and Chesapeake Bay results.*

Action: *As noted previously, no action was taken because summaries from all ancillary efforts were not available before the report was finalized. The technical TMDL being developed by the LRWQCB will include the requested information.*

Comment: The major focus of the LTDAS study was to estimate direct, dry and wet deposition of nutrients and PM to Lake Tahoe. It would be interesting and useful to compare the deposition estimates from the LTDAS study to values calculated in a photochemical grid model. The LTDAS calculated deposition rates should be more accurate than grid model results because the LTDAS estimates are based on ambient measurements of species concentrations and meteorology data. Moreover, the LTDAS adopts several detailed corrections to the deposition algorithm (e.g., corrections for the 20% of the lake surface near shores in Section 4.3.1.6). However, it would still be useful to compare the detailed, measurement based estimates of LTDAS with air quality models to see if they are generally consistent, and also to assess the possible usefulness of air quality model simulations for projecting future changes in N deposition to Lake Tahoe that may result from emissions controls both in upwind areas and locally. Because of the complex interaction of O_3 photochemistry and the conversion of N between different forms, and the very large differences in deposition velocities for different N species, future changes in N deposition can not be estimated simply based on changes in emissions inventories. It will be necessary to perform air quality model simulation using ozone-VOC-NOy photochemical and aerosol transformations. Grid models are now being operated for long periods for California, including the 2002 period of the LTDAS study, and the grid model results should be compared with the LTDAS estimates. A widely use grid model, the Community Multiscale Air Quality (CMAQ) model uses the same deposition algorithms as those developed by Byun and Dennis that were referred to and used in the LTDAS approach, so the results should be comparable in some respects (with any differences resulting from errors in model simulated values or errors & missing data in the meteorology and concentration data). Annual modeling studies for calendar year 2002 are also being performed using ENVIRON Corporations CAMx model. The calendar year 2002 simulations are being funded by the Western Regional Air Partnership and included CMAQ and CAMx simulations with grid resolutions varying from 4-km to 36-km. Another advantage of using the grid model is that it will provide simulated concentrations over the lake surface, although the lack of measurements above the lake poses a problem for model validation. In any case, future studies should use a combination of both measurements and grid model simulations.

Response: *The staff originally hoped to take advantage of current modeling efforts for other projects with domains that overlap the Tahoe Basin. In the long term, LTADS staff would like to see Tahoe-specific photochemical (with PM) modeling capabilities developed. However, the modeling efforts concurrent with LTADS have not been fully validated and results for Tahoe will not be available in time for this report. Even if the modeling results had been available, staff would have had limited confidence in the results because: 1) the modeling grid scales are large compared to the processes in the Tahoe Basin and so would poorly capture local air quality and* *meteorological effects, 2) the Tahoe Basin is in complex terrain near the eastern boundary of the modeling domain where the models would not have had their performance critically evaluated, and 3) the modeling emission inventory for the Tahoe Basin is based on standard defaults that poorly represent emissions in the Tahoe Basin.*

Action: *No action taken; the staff did not have modeling results to contrast with the LTADS deposition estimates. The staff included more comparisons of the deposition estimates from LTADS and the on-going TRG surrogate surface samplers.*

Comment: The report is exhaustive in its detailed analysis of the data and the methods used to estimate dry and wet deposition. As such it represents a major accomplishment and will be the basis for future efforts to develop more accurate estimates of nutrient and PM deposition in Lake Tahoe. Finally, it would be helpful to name the individual authors who contributed to each chapter of this report, both to credit them for their work and as a point of contact for future inquiries into the work.

Response: *The staff modestly agrees.*

Action: *The staff identified the main authors/contacts for each chapter in a paragraph below the Table of Contents as the Acknowledgements section was already a full page.*

<u>Specific Comments</u>

Comment: Page 1-1, last paragraph. Given the importance of agricultural and livestock operations as a source of nitrogen (N) and fine particulates, these sources should be mentioned in the list of likely sources of PM and nutrients.

Response: *The staff acknowledges the potential role of global and regional transport as sources of gases and fine particulates. Although agriculture and livestock operations are small in the Tahoe Basin, they are very large in the San Joaquin Valley. Staff is particularly concerned about the role of ammonia transport as ammonia was the largest contributing specie to nitrogen deposition. Staff had included some discussion regarding emissions in Chapter 7 in the draft report.*

Action: *Because of their potential significance, the staff included an allusion to these sources in this paragraph of likely sources.*

Comment: Page 1-25, Section 1.6.9: The description of the Multi-Isotope Ratio measurements is very vague. It mentions unique, new research tool based on “quantum mechanical processes” but does not provide any sort of description or references for the new method. This section should be rewritten to provide a more professional and technically complete description of this research activity.

Response: *The staff acknowledges that this summary is vague because it is based on material in the research proposal. The investigator has encountered difficulties during the research and the ARB has not yet received a final report.*

Action: *The staff revised the text with material from a progress report that better articulates the work performed and the objective of this research.*

Comment: Page 4-1, second paragraph: "This is one of several assumptions that are intended to provide a conservatively large estimate of dry deposition." The above statement implies that this study will be biased to over estimate nutrient deposition. It would be better to provide the most accurate estimate possible and also to include a more conservative, upper range estimate for use in planning purposes. The question of bias is also discussed on page 4-40, Section 4.4.

Response: *Other reviewers have expressed similar concerns about staff's statements, the implied upward bias to all estimates, and the type of analysis and presentation likely to be of most benefit to the planners.*

Action: *The staff clarified their statements.*

Comment: Page 4-1, last paragraph. Will data collected in the LTADS study be provided to the RWQCB and its contractors for estimating dry and wet deposition to land surfaces? It might be useful to provide more details here about future interactions between the ARB and the RWQCB for integrating their studies.

Response: *The staff had some limited interaction with the watershed modelers, primarily at the beginning of the project. ARB provided the refined atmospheric deposition (dry and wet) numbers to the TMDL coordinators. The staff continues to answer questions as they arise. Staff participated in an LTADS workshop in December of 2005 to share results with the Tahoe community, to answer questions, and to participate in guiding the direction of future research based on questions that LTADS was able and not able to answer. ARB and the State Water Resources Control Board sponsored their first joint workshop on atmospheric deposition and water quality in February of 2006. The objective was to facilitate improved understanding, coordination, and integration of the overlapping environmental concerns of the two State Boards, the local boards and agencies, and the public.*

Action: *Staff included some discussion at the end of Chapter 8 in the final report noting that LTADS helped fill in a few pieces of the water quality puzzle but that some questions and uncertainties remain. The recent activities, in which ARB staff participated, to support the water quality community and to reinforce the need for multi-media coordination in addressing environmental challenges was also noted.*

Comment: Page 4-2, 2nd paragraph: "Some of the nutrients deposited over land would be assimilated before reaching the Lake." Is it possible to provide an estimate or reference to the fraction of land deposition that would not reach the lake?

Response: *The staff focused their efforts on improving direct dry deposition estimates to the Lake. As such the efforts focused on near shore and on-lake measurements. A similar effort to estimate indirect atmospheric deposition would be much more complicated with respect to instrument siting (closer to local influences), faster deposition rates (due to more vegetation, buildings, and moving vehicles), and assimilation of nutrients into the biosphere. Because the* *soil in the Tahoe Basin is relatively sterile, much of the nutrients deposited in the vicinity of plants would be utilized during the growing season and its migration to the Lake significantly reduced. ARB staff has no particular expertise in assessing the fraction of the indirect deposition that might make it into Lake Tahoe and will leave that assessment in the capable hands of the watershed modeling experts.*

Action: *The staff included some additional qualitative statements regarding indirect deposition in the final report but did not make quantitative statements.*

Comment: Page 4-67, and Tables 4-12 to 4-14: The abbreviation "MT" is typically used for "Mega Tons (not Metric Tons), so the use of MT is very confusing here. (I spent quite a bit of time reviewing chapter 4 to see where the estimate of approx 150 millions tons was calculated before I went back to the exec summary and saw that it was 150 tons). It would also be helpful to include more details on the calculation on page 4-67, e.g., the Lake surface area for open water and near shore. Other ways to describe "metric tons" would be the spelling "tonnes" or long tons (as opposed to US units of 2000 ponds, spelled "ton" or "short tons").

Response: *The staff followed the practice that other Tahoe groups have used.*

Action: *The staff clearly defined MT (or wrote out) in the final report. Staff also included more details on the assumptions and calculations in Section 4.7 of the final report.*

Comment: Page 6-2, first line starts: "Among nitrogen species, ammonia was presumed plentiful and ubiquitous in nature" and therefore NH_3 was not targeted for study. This statement is probably incorrect. Although there are NH_3 emissions from animals and it is possible that soil microbial processes are an important source of biogenic NH_3, there are large uncertainties in those emissions and they are likely small in comparison to anthropogenic NH_3 sources from agricultural and livestock operations in central CA. Study of the transport and fate of the anthropogenic NH_3 emissions should be a key topic of the LTADS study. Because NH_3 had a high deposition velocity it is likely that much of the central CA NH_3 emissions deposit out (or are converted to aerosol ammonium nitrate). Moreover, the abundance of NH_3 effects the gas-aerosol equilibrium of H_2SO_4, HNO_3, NH_3 and aerosol sulfate and nitrate, and NH_3 can thereby affect the lifetime and transport of HNO_3. The same arguments used in the report to show that transported HNO_3 has a small effect on Lake Tahoe might also apply to the transport of NH_3. Nonetheless, the sources, transport and fate of NH_3 should be considered as part of this study.

Note that the importance of this is highlighted on page 7-3, section 7.1.1. where the authors state that" NH_3 was found to be the primary component of N deposition to Lake Tahoe."

Response: *The staff acknowledges uncertainties in the measurement of NH_3 and that more research may be needed to understand the origin and fate of ammonia in the Sierra Nevada. Because the limited aircraft measurements suggested nearly as much NH_3 aloft as near the* *earth's surface, staff assumed that the prodigious amount of NH_3 emissions upwind might transport to the Tahoe Basin. Aerosol modeling efforts may help inform.*

Action: *The staff took a closer look at the data and available information to confirm that the statements are reasonable and appropriate.*

Comment: Page 6-4: In the discussion of the chemistry and the list of the chemical reactions it is important to also include and discuss the hydrolysis of N_2O_5. Hydrolysis of N_2O_5 to produce 2 HNO_3 by either gas phase or heterogeneous reactions is highly uncertain but is expected to be an important source of converting reactive NOx to HNO_3, especially during the winter time. This could effect the lifetime and transport distance of NOy species.

Response: *The staff is uncertain about the amount and significance of N_2O_5 at Tahoe.*

Action: *The staff included N_2O_5 and its chemistry in the discussion for completeness.*

Comment: Page 6-13, first paragraph: The text states that "almost all the precipitation falls as snow, thus wet deposition is the only sink of NOy during the winter". This statement is incorrect and might be a typo? Did the authors intend to say that there is only dry deposition? I expect that there would be both wet and dry deposition during the winter (certainly the dry deposition of HNO_3 to a snow covered surface is rapid) so it would be best to delete the above statement.

Response: *This statement is from a contractor report but is incorrect as pointed out.*

Action: *The staff deleted this statement and the complete sentence with it.*

Comment: Page 6-20, Section 6.8, Conclusions and Implications: the discussion about fire is somewhat confusing, and I'm not certain of the significance or accuracy of the statement that "Total reactive nitrogen in the region is likely at a maximum during the summer". Is this because the fires occur during the summer, or because air masses are advected away or dispersed more rapidly in the winter? Or does "the region" only refer to the Lake Tahoe area and not the urban areas? This section could be rewritten to elaborate on this and the significance of the fire emissions.

Response: *The staff concurs that the paragraph is not clear. No wildfires occurred during LTADS. The Gondola Fire that occurred before LTADS did not have an obvious impact on the measurements from the routine monitoring network. Analysis of air quality impacts during prescribed burns during LTADS was beyond the staff resources available. As noted elsewhere, the fire impacts can range from minimal to very significant depending on a large variety of factors.*

Action: *The staff clarified the paragraph in the final report but did not include more discussion of fires and their potential impacts.*

Summary of the Major Comments Received from Professor Akula Venkatram with CARB Staff Responses

General Comment: The results obtained by ARB can and should be improved through further analysis of the extensive data set that has been collected.

Response: *The staff agrees that opportunities exist for more extensive analysis of the LTADS data. However, staff does not have the resources nor does LTADS have the high priority necessary (compared to other air quality commitments) to justify substantial additional analyses by the staff. The staff has tried via the recommendations chapter, workshop, and personal communications to encourage others to conduct the research pertinent and necessary to their specific needs.*

Action: *The staff revised the final report to clarify, refine, and better document the methods and conclusions presented but did not add extensive new data analyses.*

Comment (Ch 2 - Atmospheric Processes)**:** This section provides information on wind, temperature, and precipitation patterns in the Lake Tahoe basin. Although the discussion shows how this information is relevant to the objectives of the project, most of the conclusions are based on qualitative arguments. Examples of important conclusions that require quantitative support are:

1. *1st paragraph, page 2-48.* Materials must mix down some 700-1000 m or more to reach the Lake's surface.

2. *Same paragraph.* The high pressure zone frequently creates temperature inversions over the land that might inhibit the vertical exchange of pollutants. However, the thermal mass of the Lake may be sufficient to induce vertical mixing…….

3. *1st paragraph, page 2-49.* Emissions originating from outside the basin will have much less opportunity to interact with the Lake.

The large number of figures on wind roses and mixed layer heights can be cut down considerably by using the information explicitly in a mathematical model. The measurements made with the RASS, radar wind profiler, and mini-sodar should be used to quantify deposition or estimate source-receptor relationships using models that produce quantitative results.

Response: *The staff concurs that additional documentation and articulation may be needed to develop the conclusions. The staff chose to make primarily qualitative arguments because the staff wished to make the assumptions and subjectivity more obvious to the end user. It was not logistically feasible to collect meteorological data and pollutant concentrations with a spatial density commensurate with the complexity of the topography and the meteorological fields. The only available meteorological observations at an altitude above 100m agl are those from the radar wind profiler and RASS operated at the South Lake Tahoe Airport. Thus, if a mathematical model were used to infer concentrations, input data would have to be assumed.*

Staff did not think it would be feasible to adequately communicate to the end user the subjectivity involved in the assumptions (e.g., input data) necessary to perform modeling.

Although the report does provide some quantitative summaries of inferred meteorological conditions, that text is closely linked to caveats about how observations were used and extrapolated and so that the uncertainty is communicated. For example, frequency distributions of mixing depths over the Lake and over land areas were inferred by season and hour of day, from hourly temperature observations aloft over SLT Airport, water temperatures, and air temperatures at Sandy Way. The inferred mixing depths were summarized in Tables 2-1 and 2-2 and the text contains caveats about the spatial extrapolations used to generate those values presented. Thus, the qualitative statements cited as examples can be compared with the reported observations. Similarly, detailed summaries of observed winds were also presented. However, as acknowledged in the report, spatial interpolations in complex topography and across the land-water interface may be tenuous. The reliance more directly on the available observations highlights the difficulty of obtaining measurements with a density commensurate with the spatial and temporal complexity of this area.

Staff chose not to implement models for the purpose of this report for several reasons. A key concern is that input data are limited. Emissions inventory information specific to the Tahoe Basin is generally not available nor are observations aloft of concentrations which would needed to characterize initial conditions and boundary conditions. Air quality model results would be sensitive to assumptions regarding concentrations aloft, wind fields, and spatial variations in vertical mixing. Staff thinks that these inputs to an air quality model would need to be assumed and could not be adequately evaluated against observations. A related concern is that the TMDL schedule did not allow sufficient time to process and quality check observations of winds aloft that would be expected to serve as model inputs. Another concern is the question of evaluating model performance in complex terrain with respect to prediction of trajectories and mixing depths. Available data for model performance evaluation (MPE) is very limited relative to the spatial complexity of the topography and meteorological fields in the Tahoe Basin. While the use of models would help to indicate what <u>might be happening</u> in the intervening areas without measurements, staff is concerned about the tendency for the resulting predictions to be over-valued. First, caveats about the limitations of the available inputs and the implications of those limitations are difficult to communicate effectively and, second, those limitations and uncertainties seldom remain linked to the model results.

Staff agrees that modeling would be helpful as a research tool to provide additional insights regarding the implications of assumptions and to guide future measurement programs. However, without more certainty regarding the model inputs (data resources) and performance in a complex setting, staff thinks that providing model results to an end user not intimately familiar with the complexities and uncertainties might not be helpful and possibly misleading.

The meteorological plots in the report summarize actual events and present climatological norms. As such, they are useful as constraints in the development of conceptual models. Application of mathematical models removes the end user of the information a step away from the extrapolations involved and invites the assumption that the performance of the models is adequate in the complex mountain topography of this setting. Modeling was one of the original intents of collecting the data. However, modeling that could be relied upon for accurate predictions of deposition would require a greater level of effort than exploratory modeling such as sensitivity analyses. The input data would need to undergo additional validation and *emission inventories remain to be developed. Further, the meteorological and air quality models would need to be run and validated before their deposition predictions could be relied upon and source-receptor relationships identified with confidence. This would be a 2-3 year effort and was not feasible within the timeframe for the TMDL effort.*

Action: *The staff clarified their statements and provided additional explanation and documentation of the analyses and conclusions. Staff linked qualitative statements to the supporting observations and to any calculations used to draw inferences from those observations, but did not add modeling to the report.*

Comment (Ch. 3 - Data quality and summary of ambient concentrations)**:** Section 3.2.6 describes several experiments to examine the variation of near surface concentrations as air flows from the shore onto the lake. The major conclusion is that 'concentrations over the lake declined rapidly within a short distance of the shoreline'. Because this reduction is not accounted for explicitly in the deposition calculations, the report points out that deposition is overestimated. ARB needs to refine their calculations to demonstrate that the extensive data set that they have collected can be used to improve upon the zeroth order estimates that can be made with a much smaller set of measurements. Such refinements should rely on dispersion and deposition models to avoid the ambiguities of qualitative arguments.

Response: *Most of the short-term measurements were made directly downwind of major sources or urban source areas and may not represent concentration gradients over greater distances downwind. Mathematical models could be used to extend analysis of the short-term observations of spatial and temporal variations in concentration. However, without key inputs (e.g., concentrations aloft and the depth of vertical mixing over the lake) and additional meteorological and air quality observations for MPE, the results would have less value for predicting annual deposition. Staff sees value in the suggested analyses as an independent research effort but is not able to pursue such an effort or incorporate such analysis as part of this report.*

Action: *The staff used the available observations of spatial variations in PM concentrations by size fraction to estimate lower concentrations on the lake compared to concentrations observed at the urban sites. The resulting lower estimate for dry deposition is reported in the main body of the final report. The initial estimates based on no depletion of concentrations over the lake were moved to an appendix.*

Comment (Ch. 3 - Data quality and summary of ambient concentrations)**:** Section 3.2.6 Let me illustrate how a simple model can provide insight into the deposition calculations.

Assume that the concentration is well mixed through a layer of thickness h_o at the shoreline before the air flows onto the lake. As the pollutant is deposited at the lake's surface, the pollutant layer will grow vertically in response to turbulence. A mass balance on this layer is given by:

$$\frac{d}{dx}(UhC) = -v_d C, \qquad (1)$$

where U is the wind speed in the pollutant layer, and v_d is the dry deposition velocity. For simplicity, assume that U does not vary with downwind distance, and that h, the vertical extent of the pollutant layer is given by:

$$h = h_o + \frac{\sigma_w x}{U}, \tag{2}$$

where σ_w is the standard deviation of vertical velocity fluctuations. Substituting Equation (2) into Equation (1) and integrating yields

$$C = C_o \left[\frac{h_o U}{h_o U + \sigma_w x} \right]^{(1+v_d/\sigma_w)}. \tag{3}$$

We can show that the deposition rate, D(x), over a distance x from the shoreline is

$$\frac{D(x)}{Q} = 1 - \left[\frac{h_o U}{h_o U + \sigma_w x} \right]^{v_d/\sigma_w}, \tag{4}$$

where Q is the material flux into the lake. If $v_d=0$, no deposition occurs. For highly soluble gases, such as NH_3 and HNO_3, $v_d \approx \sigma_w$, so that Equation (4) becomes

$$\frac{D(x)}{Q} = \frac{\sigma_w x}{h_o U + \sigma_w x}, \tag{5}$$

and the relevant scale over which all the pollutant is deposited is

$$X_d = \frac{h_o U}{\sigma_w}. \tag{6}$$

For large particles, $v_d >> \sigma_w$, and the corresponding removal scale is

$$X_d = \frac{h_o U}{v_d}. \tag{7}$$

If we plug in some representative values for the variables in Equation (6) and (7), we find that pollutants being advected over the lake are removed completely within a few kilometers from the shoreline. This means that deposition calculations can be refined by measuring the advection of materials over the lake rather than making better estimates of deposition velocity. The inflow can be estimated by measuring simultaneous profiles of concentration and velocity at selected locations along the shoreline.

If we assume that deposition rate is controlled by the mass inflow into the lake, the total deposition rate over the lake is given by

$$D_T = 2\pi R C_o h_o U, \tag{8}$$

if we assume a circular lake with radius R. If we compute the deposition rate using a deposition velocity over the lake, the value is

$$D_F = v_d \pi R^2 [C_o], \qquad (9)$$

and the ratio of the 'true' to the 'false' deposition rates is

$$\frac{D_F}{D_T} = \frac{v_d R}{2Uh_o}. \qquad (10)$$

If we take v_d=1 cm/s, R=15 km, U=3 m/s, h_o=10 m, the ratio works out to be about 2.5, implying an overestimation by the ARB method. On the other hand, if h_o is actually 50 m, the ratio becomes 0.5 and the deposition might be actually underestimated using the deposition velocity method advocated by ARB. The point here is that the ratio can acquire a range of values depending on the concentration and velocity profiles in the air mass flowing onto the lake surface.

Response: *The staff appreciates the sharing of this mathematical model and agrees that this and similar models are useful for illustrating physical principles and exploring implications of assumptions. Staff understands that this is shared as an illustration of the utility of such models and that the illustration could have been made with a range of assumptions.*

Staff is hesitant to assume concentrations above the height, h_o, which defines a vertical limit for mixing of the local emissions. However, based on limited measurements and inferences, in the opinion of staff, it is probable that concentrations of ammonia, nitric acid, and fine particles (and possibly larger particles) are often not negligible above the local mixing depth. The observed temperatures suggest that during drainage flow there is shallow mixing over land and at the shoreline but deeper mixing over the Lake.

Lacking vertical profiles of concentrations and winds aloft at the shoreline, and having had some experience in attempting to acquire monitoring sites in this area, the staff can only agree in principle with the paragraph that follows equations 6 and 7:

"If we plug in some representative values for the variables in Equation (6) and (7), we find that pollutants being advected over the lake are removed completely within a few kilometers from the shoreline. This means that deposition calculations can be refined by measuring the advection of materials over the lake rather than making better estimates of deposition velocity. The inflow can be estimated by measuring simultaneous profiles of concentration and velocity at selected locations along the shoreline."

Some limited aircraft measurements were made at higher altitudes, but profiles of concentrations through and above the mixed layer were not feasible within the resources of LTADS. Irrespective of any budget or schedule considerations, based on the experience of LTADS, it would be somewhat problematic to access representative sites for profile measurements at Tahoe.

Staff questions calculation of a lower limit for deposition based on this model. There seems to be an implicit assumption, which staff thinks is questionable, that all material entering the mixed layer over the lake is removed by deposition to the lake.

Staff has concern regarding use the terms "true" and "false" in this comment. Staff understands the model and discussion was intended to illustrate the utility of this and similar models for exploring physical principles and assumptions about input variables (and that it was not intended as a definitive treatment of deposition at Lake Tahoe). Input variables were assumed, not measured, and some simplifying assumptions were made. Although these limitations are clearly stated in the comment, use of the terms "true" and "false" to discriminate between results may be confusing for some readers.

Action: *No action taken.*

Comment: Further analysis of the data should be conducted using appropriate dispersion/deposition models, which we have shown can provide insight into the governing physics and can thus guide the design of experiments needed to quantify uncertainty in the deposition estimate. It is possible that ARB has all the measurements to conduct a more refined analysis.

Response: *The staff agrees that models can help to describe the physical principles and the effects of alternate assumptions. Thought experiments or modeling experiments (i.e., based on limited or assumed inputs) would likely be useful for planning further studies. Although such analyses would be useful for guiding the design of further experiments, ARB staff does not have resources or plans for additional measurements at Tahoe.*

For providing conclusions regarding estimated rates of deposition, staff has purposely used very basic assumptions without models so as to rely more directly on the observational data and require the end users of the estimates to be more aware of any extrapolations those estimates contain. The concern of staff is that, lacking a higher density of observations (including the vertical dimension) in this setting, model predictions would be more suggestive than definitive.

Measurements of the vertical distribution of pollutants are both difficult to make and important to developing firm conclusions. We expect that air pollutant concentrations aloft are likely variable spatially and temporally due to the varied terrain, distribution of emission sources, and complex meteorological processes. Lacking key input data and more extensive observations for model validation, model results could be problematic because they would imply more precision, or even accuracy, than warranted to the end user of the information.

Experiments using models to test the effects of different assumptions are of great research interest and are useful for planning future efforts. However, for providing information to a client involved in regulatory activities, the staff would prefer to avoid a perception of precision without strong supporting data to increase the certainty and accuracy of the conclusions.

Studies in the Tahoe Basin are especially constrained by the complex terrain and resultant spatial complexity of both the meteorology and the emissions. There are also significant constraints in the form of very limited access to suitably located potential monitoring sites.

Action: *The staff provided more analysis and description of the short-term observations, both on land and on the Lake, to provide a better picture of the spatial variation in pollutant concentrations at the surface. Although experiments using mathematical models and assumptions about vertical mixing and the spatial distribution of concentrations could be useful, they were not conducted due to time constraints and because the speculative nature of inputs* *would limit the value of the modeling results. Modeling exercises are not planned and should not be conducted and disseminated until more definitive inputs are available.*

Comment (Ch. 4 - Dry Atmospheric Deposition)**:** The dry deposition rate over the lake is computed using

$$D = \int_{Lake} v_d C.dA \qquad (11)$$

ARB estimated the concentration, C, from shoreline measurements, and they do point out that this might lead to overestimation. As shown earlier, this might not be true.

The deposition velocity, v_d, is computed from shoreline meteorological measurements although the roughness length and temperature corresponding to the water surface is incorporated into the calculation. The actual deposition velocity is a weighted average of the shoreline value and the over-water value.

Response: *A higher deposition rate than estimated in the initial ARB approach would only occur if the concentrations were higher over the lake than at the monitoring sites or if the deposition velocities were higher than estimated. A decrease in concentration with distance from the urban source areas is most likely, especially for coarse and large PM. Thus, the staff maintains that it is more likely that the initial ARB approach results in overestimation because it assumed the near-source (shoreline) concentrations are constant over the whole lake.*

The mathematical analysis provided in a previous comment and referred to in this comment depends on assumptions about the depth of mixing at the shoreline and concentrations at higher altitudes over the land and the lake. It also assumed that wind speed does not change with distance downwind. This simplifying assumption (which we also implicitly employed in the LTADS estimates) generally is not the case over Lake Tahoe due to convergence during the common offshore flow periods of density driven drainage flows.

The staff recognizes that, if there are observations or other means to ensure confidence in the assumptions, the suggested analysis can provide an upper limit estimate of the deposition. However, the resources required to obtain more detailed data would be significant.

Action: *The final report states that the meteorological measurements used to estimate deposition velocities were obtained on piers and buoys. The discussion of the possible influence of on-shore roughness elements on these observations was expanded.*

Staff also moved the initial estimate of dry deposition (which conservatively assumed no depletion of concentrations over the lake) to Appendix M of the final report. A lower estimate of dry deposition, based on assumption of modest depletion of PM and phosphorus concentrations over the Lake, is provided in Chapter 4. The text of Chapter 4 links the two estimates and invites comparisons.

Comment: The equations used to compute atmospheric resistance assume horizontally homogenous conditions (large uniform fetches). They are not likely to apply to Lake Tahoe, which is surrounded by trees. Calculations of deposition velocity might have to account for the fact that meteorological measurements were made below the tree canopy height; using a roughness length of 1 m does not account for the completely different nature of the flow below the canopy. The similarity equations might apply to heights of about 2.5 times the height of the canopy, and only when they incorporate the displacement height. The extensive literature on canopy flows should be consulted before the data are reanalyzed.

The similarity equations used to compute R_a are also not likely to be valid in the transition region downwind of the shoreline where most of the deposition occurs. Furthermore, the wind speed over the lake surface might be higher than that measured at the shoreline.

The deposition velocities of soluble gases and small particles are sensitive to the surface friction velocity, u_*. ARB has used the similarity wind profiles to estimate u_*. As indicated earlier, they might not apply to the Lake Tahoe situation. This explains why ARB was forced to set a minimum limit of 1/6 (s/cm) for R_a; the estimated u_* was unrealistically high. Some of these problems could have avoided by measuring the surface shear stress directly using a sonic anemometer. If this was problem, u_* could have been inferred from measurements of vertical velocity fluctuations, σ_w. It appears that ARB does have access to turbulence data that can be used to refine calculations of atmospheric and laminar sublayer resistances, both of which are sensitive to surface friction velocity.

Response: *The wind data used in the LTADS analysis were primarily from buoys and piers (provided through the generosity of Professor Geoff Schladow of UC Davis). These meteorological sites were well-chosen to represent conditions on the lake. The buoys were several kilometers from land and the pier stations were generally located more than 100 meters from large obstructions, such as trees or buildings, even for a wind direction that is directly offshore (during drainage flows). Although some of the pier observations may be within a transition zone, comparison between buoy and pier observations (e.g., wind speed frequency distributions) suggests that the pier measurements were similarly well-exposed and not greatly affected by upwind forests or buildings.*

Action: *The staff provided additional information on the meteorological sites. The staff also enhanced discussion of the limitations of similarity theory as it relates to estimation of friction velocity from wind observations obtained downwind of obstructions. The report also explains the reasons for using assumed maxima for aerodynamic conductance (inverse of aerodynamic resistance) in the near-shore zone during offshore flow and that the assumed values were chosen to be conservatively large.*

Comment: I have some minor comments that should be addressed if ARB chooses to revise their deposition calculations:

1. The comparison of R_a computed using the Byun and Dennis method with that based on Equation (4.12) is not useful because the bulk aerodynamic method does not account for stability effects.

2. It is preferable to rely on peer-reviewed literature rather than on the CALMET user's manual for the deposition equations.
3. Equation (4.8) should be first expressed in terms of the surface heat and momentum fluxes before the parameterizations for the fluxes are inserted into the definition.
4. Equation (4.6) has some errors: the log term should not have $(z-z_o)$, while the second term be $4.7(z-z_o)/L$. The equation should be consistent with Equation (4.10).
5. It is claimed in several places (first sentence of section 4.3.1.6) that the computation of R_a assumes a logarithmic wind profile. Obviously this is not true as Equation (4.6) shows; there is a logarithmic term but there are other terms that depend on z/L.
6. Why present Equation (4.15) if it was not used?
7. How were the results of section 4.5.4 (page 4-53) used in the calculations of deposition rates? Why are the conclusions relevant?

These minor comments should not distract attention from my major concerns that the similarity equations might not apply to the inhomogeneous conditions of Lake Tahoe, and that using Equation (11) might lead to errors in dry deposition estimates. These issues can be settled by making some flux measurements using sonic[anemometer]s. This data might be already available to ARB.

Response/Action:

1. In applying the Byun and Davis method with the Tahoe meteorology, the results appeared to be more sensitive to wind speed than to thermal effects. As noted, the bulk coefficient method does not include thermal effects. The final report retains a comparison with the bulk coefficient method results but includes additional discussion of the limitations and lack of treatment of stability effects by that method.

2. Original peer-reviewed references were added so as not to rely solely on the CALMET users' manual as a reference for any points. However, references to the manual were also retained for convenience.

3. A short appendix (Appendix F) was added to provide discussion of the formulation of the Monin-Obhukov length and Equation 4.8.

4. Equation 4.6 was corrected.

5. The text was modified to acknowledge the additional terms associated with non-neutral stability.

6. Some readers may be familiar with Equation 4.15 as it has been widely used and referenced in the past. It was included because staff thought it would be helpful for alerting those readers to the error and providing a familiar point of reference before moving to the corrected formulation.

7. In Section 4.4 of the report, the results of short-term experiments to characterize the spatial and temporal variability in PM concentrations are discussed. This discussion has been expanded. With this reorganization and expansion, this information precedes discussion of the *assumptions and potential bias in the LTADS estimates. Thus, it is positioned so that it can serve as a reference for readers as they review the assumptions made. Although the experiments were brief and spatially limited, they have value for assessing the validity of the assumptions.*

Several reviewers suggested that a less conservative deposition estimate based on a more realistic assumption of lower concentrations on the lake would be more useful. Such an estimate was made in the final report, using assumptions that derive in part from the results of the short-term experiments.

Comment (Ch. 5 - Wet Atmospheric Deposition)**:** ARB has used available data to make first-cut estimates of wet deposition over Lake Tahoe. These estimates are based on a formulation presented on the bottom of page 5-4. As far as I know, this formulation is not based on any work reported in the extensive literature on wet deposition. It might be misleading to call it a "first principles" approach. It incorporates several parameters whose values are assumed rather than determined from first principles. For example, the depth of the layer that is washed out is taken to be 3000 m for transported pollutants, and 700 m for in-basin pollutants. There is little justification for these assumptions. Furthermore, taking surface concentrations to represent average concentrations over these deep layers is a risky proposition.

The factors HW (horizontal washout fraction) and VW (vertical washout efficiency) are varied to produce a range of wet deposition estimates. Because we know almost nothing about these parameters, does this range of estimates have any meaning? The agreement with TRG bucket measurements might be accidental.

The conventional approach to estimating the wet deposition rate is based on the parameterization:

$$D_w = C_a w_r h p \ , \qquad (12)$$

where C_a is the air concentration averaged over the height of the storm, h, and w_r is the washout ratio defined as the concentration in precipitation divided by the concentration in air, and p is the precipitation amount. There is great deal of uncertainty in estimating w_r, which depends on a host of variables such as droplet size distribution, solubility of the pollutant in water, and aqueous phase chemistry etc. Seinfeld and Pandis (1998) provide details of the "first principles" approach to computing wet deposition. Estimates of wet deposition can be made with empirically derived values of w_r. However, we still need reasonable estimates of C_a and h. I would put more trust in wet deposition 'measurements' in buckets than in theoretical calculations.

Response: *Staff agrees that the characterization of the wet deposition estimate as being based on a "first principles" analysis is misleading and implies more credence to the results than is justified. The conceptual model might better be characterized as a "back of the envelope analysis based on atmospheric principles". However, the ARB approach is very similar to the approach suggested except that the staff model was constructed to provide differentiation of local and regional pollutant sources. In addition, because the amount of wet deposition is greatest during the beginning of a storm, ARB believes that precipitation frequency is a better* *indicator of the deposition amount than the amount of precipitation is (e.g., a 3" storm does not wash out 3 times the material that a 1" storm does).*

The staff agrees that estimating an average concentration throughout the precipitation layer is "a risky proposition". The conceptual model assumes good mixing of the pollutants aloft (characterized by Big Hill data) and good mixing of local pollutants (characterized by 4-quadrant mean of in-basin data) below the ridgeline. Concentrations during organized storms were discounted by using the cleanest 2-week average concentrations observed during those seasons (winter and spring).

The staff agrees that the wet bucket measurements are more trustworthy than the ARB wet deposition estimation. The reasonable agreement between the bucket measurements and the staff estimates helps to confirm the validity of the pollutant concentrations used in both the wet and dry estimates by ARB. Of course, the ARB methodology is still needed to generate an estimate of wet deposition of PM which the bucket measurements cannot provide.

Action: *The staff provided more documentation and better articulated the rationale behind the transported and local components of the wet deposition (e.g., based on the depth (cloud top minus cloud bottom) of the typical thunderstorm in the Sierra Nevada).*

Comment (Ch. 6 - Air Pollution Transport)**:** This is a useful discussion of the processes that govern transport of pollutants from different sources into the Lake Tahoe basin. However, I do not see how one can estimate the transport component of the pollutant budget without some sort of mathematical model. Figures 6.6 and 6.11 present quantitative results, but they are not part of an integrated framework that a model would automatically impose on the analysis. The conclusions in Section 6.8 are useful, but without some numbers attached to them it is difficult to interpret them.

Response: *One cannot provide quantitative information on the transport component without additional detailed air quality and meteorological data. Even though models provide quantitative results, the numbers are only as good as the input data and their ability to correctly reproduce the active atmospheric processes. The primary purpose of this section was to present evidence (though incomplete and circumstantial) to counteract an in-basin mindset that all of Tahoe's problems are due to pollutants being emitted upwind of the basin.*

Action: *The staff provided more specificity in the final report, including existing additional analyses, information, and documentation to support the transport conclusions. The resources and priority were not sufficient at the time to conduct a comprehensive analysis. Additional monitoring efforts (e.g., more sites and more aloft measurements for a longer period of time) would be necessary to definitively quantify the transport impact.*

Comment (Ch. 7 - Characterization of PM and Nutrient Sources)**:** ARB conducted several studies to refine emission estimates of PM, N and P originating from roads, motor vehicles, and wood smoke. It is not clear whether these studies resulted in emission factors that could be used to construct a realistic emission inventory for the basin. Without such an inventory, it would be difficult to check consistency among different components of the pollutant budget for the basin.

On of the conclusions from the special studies is that large fraction of the PM originates from roads. This is supported with results such as those presented in Figure 7-8. My questions are: How was the flux calculated without wind profiles? Why is the depth of the plume from the road more than 400 m at a downwind distance of 1 m from the source?

Response: *Although providing insights, these limited special studies produced highly variable source profile results that were not definitive for improving the emission inventory. Additional work is needed to construct a realistic emission inventory for the Tahoe Basin. The current emission inventory for Tahoe is based on many standard assumptions that appear not to be appropriate for the Tahoe Basin. Complicating the development of an emission inventory is the bi-state nature of the basin. The current state of the inventory is only useful for crude analyses and is not appropriate for modeling and planning applications.*

Regarding the depth of the plume at 1 m from the roadway, the depth is 400 cm, not meters, and so is reasonable given the mechanical turbulence induced by moving vehicles.

Action: *The staff clarified in the final report how the wind profiles were estimated for calculating the flux of material with the optical particle counters.*

Comment (Ch. 8 - Conclusions, Lessons Learned, Insights, and Recommendations)**:** I expected to see a table that provided a pollutant budget for the basin. The components of such a budget are: Inflow, basin emissions, outflow, total wet deposition, and total dry deposition. If possible it should include dry deposition and wet deposition due to local sources to ensure consistency with local emissions.

It is clear that ARB staff recognize the uncertainties in their deposition estimates. Their suggestions to reduce these uncertainties are well thought out. I believe that if ARB does not follow up on these suggestions, the report, as it stands, will have limited value to the community.

Response: *The staff would need to crudely estimate numbers to generate a pollutant budget as the inflow, emissions, outflow, and indirect atmospheric deposition are all poorly known. The dry deposition methodology has no component that separates regional versus local components but staff will consider making a crude estimation. Staff believes the draft report had value to the planners but that the final report will be of more value because it refines estimates and helps to constrain uncertainties.*

Action: *The staff made the major revisions that will help to reduce uncertainties and make the estimates more useful for the water clarity modelers.*

Summary of the Major Comments Received from Professor Anthony Wexler with CARB Staff Responses

General Comments

Comment: The staff has endeavored to provide conservative estimates of the various sources, where they define conservative as the upper bound. But is an upper bound estimate the conservative or most useful one? Since the estimates provided here will be compared with other sources, various scenarios present themselves:

- If the CARB conservative estimate is lower than the other sources, then conservative defined as an upper bound is the correct measure – it is conservative because even if the answer is lower, agencies will be correct to direct their attention to the other sources and ignore the atmosphere as a source.
- If the CARB conservative estimate is comparable to or lower than the other sources, then conservative defined as an upper bound is not appropriate or "conservative" in that remediation measures may be applied to atmospheric sources when they may not be relevant.

I will make the case here that the LTADS conservative estimates may be an order of magnitude too high. If this is supported by the other reviewers, I recommend that the upper estimate remain as given by CARB but that the lower and best estimates be revised downward accordingly.

Response: *This is an excellent point. The staff, when needing to make assumptions or estimates, tended to use slightly conservative representations (assumptions which would tend to maximize deposition) to ensure that atmospheric deposition would not be underestimated. Staff probably over-emphasized this point in its report and created an impression stronger than appropriate. Given the uncertainties associated with the estimates (e.g., the potential impact of large particles not collected by the TWS) and given the clean air in the Tahoe Basin (e.g., ambient concentrations frequently approaching background levels), staff believes the Best Estimate of deposition is realistic. The point staff attempted to make is that atmospheric deposition is not as large as the earlier estimates with surrogate surface samplers indicated (e.g., due to contamination by vegetative debris, insects, and birds). However, the LTADS upper bound estimates clearly represent an unlikely combination of multiple extreme scenarios.*

Action: *The staff revised the report to reduce the impression that the Lower Bound and Central Estimates are overly conservative. Staff revised the deposition estimates based on other comments (e.g., increased phosphorus self absorption correction factors, pollutant depletion with advection over the Lake, discounting impact on water clarity of soluble particles) but not in response to this concern about conservative bias because, in many cases, an alternative assumption does not exist (e.g., using less than the minimum 2-week average concentration for characterizing concentrations during a winter storm).*

Comment: A key assumption for the dry deposition estimates is stated on page 4-66: "Concentrations measured near shore are assumed to be representative of both the near shore and open water areas of the lake." This is in great contrast to the data showing rapid decay in concentration near the shore (Figure 3-26, page 3-103). This is especially of concern for the large particle size factions where the background concentration may be an order of magnitude lower than the near shore value, and where the majority of the atmospheric PM resides. Using rough numbers, if we estimate the lake to be 20 km by 30 km, then the surface area is 600 km^2. If the particle concentrations decay to background over a distance of 60 m and if we assume that the midlake deposition is negligible because the deposition velocity is much lower than near-shore and the concentrations are much lower due to the aforementioned decay, then the effective area for deposition is 2(20km + 30km)x60m – the perimeter times the coastal boundary layer – giving an effective deposition area of only 6 km^2, 1/100 of the lake area estimated above. The actual area of Tahoe is more like 500 km^2 but this rough calculation is just done for order of magnitude illustration. The staff recognizes this potential overestimate in section 4.4.1 on page 4-40.

The dry deposition estimate may not be two orders of magnitude too high because the staff's deposition velocity decreases rapidly near the coast so that mid-lake deposition values are already much lower than those near the coastline. On the other hand, non-soluble particulates depositing near the shore may settle to the relatively shallow bottom before being transported to the deeper portions of the lake, even reducing further the effect of near-shore deposition (although this is beyond the scope of the current study). The staff should use the method outlined above to re-estimate the deposition assuming this rapid decay in concentration near the coast.

Response: *The staff agrees that additional consideration could be given to spatial variation in concentrations especially for PM and phosphorus. Additional observations or attempts to refine estimates of atmospheric concentrations over the lake would likely reduce deposition estimates for particles and phosphorus. In the short-term experiments with optical particle counters (OPC), there were obvious and dramatic declines in particle counts with increasing distance downwind of sources due to the combined influences of vertical dispersion and deposition. However, parsing the decline between vertical dispersion and deposition depends on making some assumptions because vertical profiles of upwind concentrations were not available. Differences (for different particle sizes) in rate of decline of concentrations with downwind distance are partly due to the interaction of vertical dispersion and any differences in their vertical gradients of concentration upwind of the source. To facilitate analysis of results from the OPC experiments, the staff assumed that the local emissions overwhelmed upwind concentrations in the vicinity of sources. Farther downwind, this assumption is less easily justified. Observations of upwind concentrations generally were not available. Compared to the locations of the OPCs during the short-term experiments, the monitoring sites used to infer concentrations for calculation of deposition (TWS and BAMs) were more distant from the local sources. Thus, gradients of concentration are expected to be less dramatic. Also, the two-week samplers integrate across periods of any wind direction and emission sources. For several reasons, including those stated above, staff did not directly apply the observed decline in particle counts downwind of the roadways to estimate on-lake concentrations.*

The contrast between particle counts on the lake and on land could be used to infer concentrations on the lake but generally the observations were made to characterize *concentrations during periods of downslope and offshore flow and to represent relatively few hours, mostly during offshore winds.*

Action: *In a revised analysis, the staff estimated dry deposition using alternative lower estimates of concentrations on the lake. The Thunderbird site is located near the east shore about 600 meters west of Highway 28. There is some local activity but the area between the highway and the site is generally forested. During the majority of hours, this site is upwind of the highway and downwind of the Lake. Thus, concentrations observed at Thunderbird should not be greatly impacted by Highway 28 (due to wind direction, distance, and likely deposition to the forest canopy). The staff used concentrations observed at Thunderbird as an upper limit for background concentrations at mid-lake.*

Comment: Wet deposition is estimated by dividing the season into times of regional and local contributions. The argument that the local contribution may be too high is the same as for the dry deposition – using near shore ambient concentrations to represent those over the whole lake is probably an over estimate by an order of magnitude or so. The argument for the regional contribution is that during frontal passages, the Big Hill concentrations of pollutions should be many orders of magnitude lower than during more stagnant events without precipitation. For the regional wet deposition contribution, staff used the average concentrations at Big Hill but during frontal passages, these concentrations should be an order of magnitude or more lower than the average again leading to an over estimate by a factor of 10 or so.

Response: *As noted, staff did use seasonal average concentrations from the Big Hill site to characterize the regional or transport contribution to wet deposition within the Tahoe Basin. The conceptual model used the seasonal mean concentrations during fall and summer when precipitation is associated with isolated thunderstorms and the transported pollutants are not necessarily washed out before arriving to the Tahoe Basin. However, the model used the minimum 2-week average concentration for the winter and spring seasons when the precipitation is associated with frontal passages where the precipitation from the organized storm has removed much of the pollutant load during its passage and pollutant concentrations are lower. Even so, because precipitation does not occur for two straight weeks, the minimum 2-week concentrations will still overestimate the actual concentrations during the frontal passage and therefore also the deposition due to regional sources of pollution. The same rationale about ambient concentrations during precipitation events also applies to the estimate of wet deposition due to local sources.*

Action: *To clarify how the data were used to estimate the wet deposition associated with regional and local sources, the staff enhanced the discussion of the wet deposition model and also included information on the ratios of the minimum 2-week average concentrations by season compared to the seasonal average concentrations.*

Comment: Remarkably, the staff estimates of wet deposition are very close to those estimated by TRG and NADP (Table 5-11 on page 5-25). The staff expresses reservations about the TRG sampler at the Wallis Residence, but otherwise no information is given about possible uncertainties in these measurements. Due to the agreement between the wet deposition calculations and the TRG/NADP estimates, I have lower confidence in my contention that the upper estimates are an order of magnitude larger than the best estimate. More information about the TRG and NADP estimates might help clarify.

Response: *The staff notes that the ARB methodologies for estimating wet and dry deposition from the LTADS measurements were very different. The only commonality was the use of seasonal concentrations observed during 2003. Thus, the uncertainties associated with the dry deposition methodology have very little to do with the uncertainties of the wet deposition estimation methodology. Therefore, the wet deposition results should not influence your confidence in the dry deposition estimates.*

Action: *The staff expanded the discussion of surrogate surface measurements by TRG and NADP in the final report and also contrasted them with the LTADS results.*

Comment: What is the possible contribution from wildfires – not addressed well in the study because wildfires did not occur during the measurements?

Response: *The staff acknowledges that wildfires can have significant local effects and, depending on the size of the fire and the meteorology, can have significant effects far downwind of the fire. The difficulty is how to incorporate a potentially large source that occurs with irregular frequency into an annual or seasonal estimate of deposition to a large body of water. Staff sought to address this "wild card" by sampling continuously throughout the year with Two-Week-Samplers deployed around the Lake, the rationale being that any irregular event, whether local or distant or large or small, would be integrated into the measurements along with the routine emissions. Although the Gondola Fire occurred in the SE area of the Basin before most of the LTADS instrumentation was deployed, the air quality impact at the long-term monitoring sites in South Lake Tahoe was quite small. It is difficult to characterize emissions from a wild fire because it has different phases of burning (e.g., hot vs. smoldering), the lofting of emissions above normal sampling equipment, the direction and dispersion of the plume due to meteorological conditions, and the inherent difficulty and safety concern of making measurements close to the fire.*

Action: *The staff did not include more discussion of the potential effects of wild fires in the final report because of the variety and uncertainty of impacts. However, the report includes references to air quality measurements and analyses by Professors Carroll and Cahill of specific smoke impacts in the Tahoe Basin.*